PHILADELPHIA, AUGUST 11, 1883.

ORIGINAL LECTURES.

CLINICAL LECTURE

ON DIABETES MELLITUS.

Delivered at the Pennsylvania Hospital, January 10, 1883,

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Reported by WILLIAM H. MORRISON, M.D.

GENTLEMEN,—In fulfilment of the promise which I made at the last lecture, I re-introduce the case of diabetes mellitus to which at that time I briefly alluded. In order that the case may be fixed on your minds, I shall again give you its history.

This woman is 50 years of age, a child's nurse by occupation; she was always remarkably healthy until last summer. She then noticed that she passed more urine and was more thirsty than was natural with her. This condition has continued until the present time. Since last summer she has lost much flesh and strength, although the appetite has continued as good as ever. Her family history is good.

Two weeks ago she caught cold, and has been coughing considerably ever since. On admission, the patient was pale, thin, and weak; the tongue was dry, red at the sides and tip, the middle being covered with a dry white fur. The mouth was dry and the gums red; the heart and lungs normal; the urine has a specific gravity of 1040, and contains a large amount of sugar, the fermentation process showing thirty-nine grains to the ounce.

She was at first placed on a placebo of aqua camphoræ in one-ounce doses, twice daily, in order that the exact quantity of urine passed might be determined. On December 29, the day after admission, she passed eighty ounces of urine. On January 1 the camphor-water was stopped, and she was ordered one grain of opium twice a day, to be increased by one grain every third day. On January 5 she was placed on a skim-milk diet. That day she passed ninety-two ounces of urine having a specific gravity of 1042. On the 6th the amount was eighty-eight ounces, specific gravity 1044; on the 7th, eighty ounces, specific gravity 1044; the 8th, sixty-six ounces, specific gravity 1035; the 9th, sixty-eight ounces, specific gravity 1035; the 10th, eighty ounces, specific gravity 1030. Up to to-day there has been progressive improvement, not merely in the diminution of the amount of urine, but also in the lowering of the specific gravity. Although during the twenty-four hours ending this morning she passed eighty ounces, the specific gravity was only 1030.

At the last lecture I showed you the distinctive features of diabetic urine. I showed you that it has a high specific gravity. I called attention to the various tests for the presence of sugar. One was Moore's test, which consists in boiling a certain amount of the urine with an equal amount of liquor potassæ, when, if the urine contains diabetic sugar, it will become dark in color, and if the condition is at all aggravated it will assume a port-wine color. I also showed the different copper tests dependent upon the deoxidation of the oxide of copper and the production of the suboxide, which is thrown down as a brick-red deposit. It was also pointed out that when a certain portion of yeast was added to diabetic urine, fermentation was set up, the effect being to form carbonic acid and alcohol and to lower the specific gravity of the liquid. There is another test, to which I did not allude,—*i.e.*, the use of subnitrate of bismuth with caustic potash. If sugar is present, a black deposit of the oxide of bismuth is thrown down. This test is not so frequently used as the others mentioned. In addition to these tests, diabetic urine has a sweetish odor, which has been compared to that of hay, roses, or violets. The taste is also said to be sweetish. The urine is also increased in amount. Instead of being fifty ounces, as in health, it is much more. In this case it has been as high as ninety-two ounces in twenty-four hours. The specific gravity, instead of being from 1020 to 1024, is much higher. Here it has been up to 1044. We find that this urine responds to all the tests and has all the characteristics of diabetic urine; and there is therefore no doubt that this woman is suffering from diabetes mellitus.

There are several diseases which, in the large amount of urine passed, resemble

diabetes mellitus. There is one in particular,—that is, diabetes insipidus. In this disease, while the amount of urine excreted is excessive, its specific gravity is low; instead of being 1040, 1050, or 1070, it is but little more than ordinary water, being only 1004 or 1005. The urine, in such cases, does not respond to the tests for sugar. There is therefore little difficulty in discriminating the two. There are other conditions in which the amount of urine is greatly increased, as in certain forms of Bright's disease. In the cirrhotic kidney there is the excretion of an excessive quantity of urine. It may be as much as is passed by this woman. The specific gravity is, however, low, and when tested for sugar there is none found. On the other hand, albumen is, as a rule, present, but this is not always true, for in certain cases of cirrhosis of the kidney it may not be found. In albuminoid disease of the kidney the urine is much increased, but it is of low specific gravity and contains albumen. In this form of disease albumen is, as a rule, readily found. There is in albuminoid degeneration of the kidney, more frequently than in cirrhosis, the presence of dropsy, notwithstanding the large amount of urine passed. On the other hand, dropsy is not a frequent symptom of diabetes mellitus, although it occasionally occurs in the last stages. There may, of course, be a temporary polyuria from the ingestion of large amounts of water, or certain diuretics. Beer and wine will also increase the amount of urine. If this condition does not last more than a day or two, it does not constitute a symptom of disease. On the other hand, it is a sign of health, for it shows that the kidney is capable of throwing off the increased amount of water ingested.

Let me now review this case. The first symptom to which she called attention in this connection was that she passed more water and was more thirsty than natural. Upon questioning her more carefully, however, I found that she had suffered before, or at all events at the same time, from a symptom which is not uncommon in these cases, and one which should always excite suspicion when it occurs in women rather advanced in life. This is some itchiness of the vulva. She also states that she lost strength and flesh, although her appetite continued good. We can readily understand this. Every grain of sugar which passes off in the urine represents a certain amount of nourishment which has failed to be assimilated.

We find, on a further examination of the history, that a short time ago she had an attack of bronchitis, for which she was admitted. Until the urine was examined, she was not known to be the subject of diabetes. The bronchitis was probably to some extent due to the fact that her weak condition rendered her more liable than she otherwise would have been to take cold.

Looking at the patient, you see that she is emaciated, although not extremely so. Her skin is usually dry and harsh, but at present, as a result of the excitement of being brought before you, she is perspiring, and the skin is moist. The tongue is slightly red, and has the coating of which the notes speak.

The principal symptom of diabetes is the presence of sugar in the urine. The presence of the sugar is rather difficult to explain. Bernard, one of the most distinguished physiologists of France, supposes that sugar is formed normally in the liver, and, in a series of experiments which he conducted, found that sugar is almost constantly present in the ascending cava and the right side of the heart, while it disappears to a great extent in the arteries. This opinion has been adopted by the great majority of observers. There are, however, a few who doubt that the formation of sugar is a normal function of the liver. They say that there is formed in the liver a substance readily converted into sugar, but that in health this change does not take place. When this substance is converted into sugar, the latter appears in the urine. In order, therefore, for diabetes to exist, there must be either an excessive production of sugar or of some substance readily converted into sugar, or else there must be a diminished combustion of these materials. There may be too much formed or too little used. In either case the sugar would appear in the urine. Diabetes is therefore not a disease of the kidney; the kidney simply excretes the sugar. In most cases of diabetes the kidneys after death are found to be healthy. In certain cases they have been found enlarged and flabby, simply from over-exertion of their normal function. Diabetes may coexist with albuminuria and kidney-disease.

In regard to the liver there is no doubt that sugar, or a substance convertible into sugar, is formed in the liver; but this does not prove that diabetes is necessarily a disease of the liver.

Bernard has gone further: he has irritated certain portions of the nervous system, and caused the appearance of sugar in the urine. If a spot in the floor of the fourth ventricle is irritated, an increased amount of urine is formed. If the floor of the ventricle is irritated at a little different position, near the end of the calamus scriptorius, sugar is formed. If both points are irritated at the same time, there are both sugar and increased amount of fluid. The disease would therefore seem to be dependent upon some irritation of the nervous system. We have further evidence of this in the fact that diabetes has not infrequently followed falls or blows upon the back of the neck, which might produce irritation of this portion of the nervous system.

Dickinson has shown that this part of the nervous system undergoes alterations in this disease. He has found by a series of careful observations on the medulla that in diabetes the vascular spaces are enlarged. It is, therefore, probable that the disease is one of the nervous system, and that the liver is affected only through the nervous system.

As I have stated, sugar appears to be a normal constituent of the blood of the ascending cava and of the right side of the heart. There are certain observers who claim that the presence of sugar in the urine is normal. They say that delicate testing will always reveal it. If it exists at all, the amount is so small and the tests required are so delicate that when the presence of sugar is revealed by the ordinary tests, diabetes may be assumed to be present.

There are various forms of diabetes. Its severity is found to vary according to the age of the patient. In the young it is a very severe disease. In the old it is by no means so severe. I have had under my observation cases of diabetes which have lasted for ten years. These occurred in people advanced in life. Three or four years ago I was consulted by a physician in regard to his mother, who had exhibited marked evidences of diabetes. Under proper treatment the amount of sugar diminished, and I believe has at times disappeared. She lost very little flesh and strength, and at no time was her condition alarming. After a time the disease again increased, but it has since again diminished. A few years ago I saw a lady who had diabetes for ten years, but who had in spite of it maintained a considerable degree of flesh.

This patient is more emaciated than is usual. As I have already said, the presence of a certain amount of sugar in the urine of a person advanced in life is by no means of as grave import as when it occurs in a younger individual. In this case the amount of sugar and of urine is not excessive. The amount of water passed may be as high as fifty pints in the twenty-four hours. In children and young adults the disease usually runs a very rapid and fatal course. In very few of these cases does decided improvement ever take place. I cannot say that I have ever seen permanent improvement secured in a young person; but in the old, improvement often follows appropriate treatment. In the young the disease not rarely runs its course within six months of its discovery.

It is of great importance to know how to treat these cases. It has been found that the amount of sugar passed is to a certain extent under the control of the patient and his physician. In certain instances the sugar will entirely disappear from the urine if the patient is restricted to a certain diet. It has been found by experiment that fatty articles and those containing starch or sugar will greatly increase the amount of sugar passed. In some instances the elimination of these articles from the diet will cause an entire disappearance of the sugar. In other cases it simply lessens the amount of sugar. This is, however, a gain, for it thus prevents the loss of flesh and strength consequent upon the passage of sugar.

In all cases where it can be borne, the patient should be placed upon a diet from which the fats, starches, and sugars—in other words, the hydro-carbons—have been removed. It is by no means easy to do this. It would be rather easier to live upon a diet of vegetables and articles containing starch and sugar than upon a diet into which these substances do not enter. Only a few vegetables can be allowed, as spinach, cabbage, and the green part of celery. Bread made from ordinary flour cannot be allowed, but the bread should be made

from bran or from almond flour. The difficulty with this treatment is that the patient soon tires of it. There can be but little variety in it.

It has been found that patients may live for a long time on milk, and that under its use the sugar frequently disappears from the urine. Cream, of course, belongs to the hydro-carbonaceous substances. Skim-milk is, therefore, better than new milk. This treatment was introduced, or at least strongly recommended, by Dr. Donkin, of England. I have used it in a number of instances, and always with a good result. A physician whom I know placed his father, who is a sufferer from this disease, upon a skim-milk diet, causing an entire disappearance of the sugar.

In regard to medicinal treatment, this is not satisfactory. There is a long list of remedies which have been recommended in the treatment of this disease, but the very length of the list shows their inefficiency.

When I was a resident in this institution, under Dr. George B. Wood, yeast was given, and with some success. The theory on which this was given was that by fermentation the sugar was converted into alcohol and carbonic acid. This did not prevent the formation of sugar. Rennet has been used in the same way, and with the same object.

Alkalies, as bicarbonate of potassium, have been employed, and have seemed to lessen the quantity of sugar passed. Certain waters containing the carbonates, as Carlsbad and Vichy, have been used with some success in lessening the amount of sugar, especially in elderly people.

Opium seems to be the remedy which goes nearer to the root of the disease than any other. This acts directly upon the nervous system, and it has the power of diminishing the quantity of urine. I give it according to the recommendation of Dr. Pavy, in doses of one grain twice a day, to be increased by one grain every third day. This has been found to be of service in all cases. Dr. Pavy has also recommended codeia when for any reason opium cannot be given. I have used it under such circumstances with success. The objection to the use of opium and skim-milk is that they are constipating.

The use of lactic acid has been advocated by Dr. Cantani, an Italian physician, and has been of service in some cases. I believe that all treatment is inefficacious if the diet is neglected. It is a singular fact in regard to diabetes, that if a patient transgresses the rules in reference to the diet, the amount of sugar which appears in the urine is much greater than the amount of sugar contained in the substance taken; for instance, a pear or a peach will increase greatly the amount of sugar. It is, therefore, necessary not only to restrict the diet, but to restrict it rigidly.

Before leaving the subject, there are certain points upon which it would perhaps be well to touch.

In regard to the method of estimating the amount of sugar present, there are several methods by which this may be done. The specific gravity will give a certain amount of information. If there is a large amount of water, and the specific gravity is high, we of course know that the patient is losing a large amount of sugar, but it does not tell us the exact amount that is being passed. This can be readily obtained by fermentation. The process is as follows. Take two conical glasses containing urine, put into one a small quantity of yeast, and set them both aside for twenty-four hours. At the end of this time the difference in specific gravity is noted, each degree lost being equivalent to one grain of sugar for every ounce of urine passed. This was done in the present case. At the end of twenty-four hours the specific gravity of the urine to which the yeast had been added was 1005, while that of the urine contained in the other glass was 1044,—the difference being 39 degrees. There was, therefore, in each ounce of this urine thirty-nine grains of sugar. By multiplying the number of ounces passed during the day by this number the daily excretion of sugar would be obtained.

The amount of sugar may also be determined by Fehling's test, by adding the solution until the blue color ceases to be altered. Each minim of the test-solution corresponds to one grain of sugar.

If the carbonic acid developed by fermentation is collected, it will also enable us to determine the amount of sugar, as each cubic inch of carbonic acid gas equals one grain of sugar. This requires, however, a complicated apparatus; while, on the other hand, the method by fermentation requires only two conical glasses, a urinometer, and a little yeast.

ORIGINAL COMMUNICATIONS.

PROFESSOR BILLROTH'S METHODS OF ANTISEPTIC SURGERY.

BY W. W. JAGGARD, M.D.

ANTISEPTIC surgical methods, as practised by Prof. Billroth, differ in many important particulars from those of English, German, and French surgeons. While the general principle of antiseptic procedure remains identical with that of Lister, several radical deviations from his methods have been made.

An American physician, upon visiting Billroth's operating-room, is at once struck by the entire absence of the spray and the substitution of carbolic acid by iodoform. These two changes exercise a measurable influence in simplifying antiseptic surgical methods.

Mosetig von Moorhof deserves the credit of introducing the iodoform dressing into Billroth's clinic, although it was used extensively as a surgical dressing as early as 1828. Miculicz, emeritus assistant to Billroth, contributed to the renown of the drug by his thorough and practical experiments, whose results were published over three years ago. When first introduced, the drug was used in substance, in large quantities, and toxic effects were soon noticed. As employed at present, in the form of gauze, the danger of toxæmia has been reduced to a minimum.

O. Weber and Billroth were the first to demonstrate that the phlegmonous and febrile symptoms after all forms of injury are due to the absorption of substances formed by chemical changes taking place in the inflamed tissues. From this important discovery resulted Billroth's open treatment of wounds. This method secured the easiest possible escape of all wound-secretions, and its results, in comparison with earlier methods of treatment, were very favorable.

Lister, following the teaching of Schwann and Pasteur, who taught that the offensive discharges of wounds were due to the presence of certain minute vegetable organisms (termed by Billroth *coccobacteria septica*), sought to prevent their contact with wounded surfaces.

The Germans ascribe to Lister merely the popularizing of a scientific fact, while they exaggerate the importance of the discoveries of O. Weber and Billroth. Upon the other hand, too many English surgeons are ignorant of the services of the latter investigators.

O. Weber and Billroth taught the doctrine that wound-fever and septic fever were different degrees of the same process, to which, in some cases, venous thrombosis and infectious embolism—multiple pyohæmia—could be added. This doctrine presents a marked contrast to the older teaching that wound-fever was due to irritation of the nerves, while pyohæmia was a miasmatic contagious disease.

I.—PREPARATIONS FOR THE OPERATION.

The ventilation of Billroth's operating-room is a matter of great importance, and is usually very thoroughly effected. When the ventilation is for any reason defective, the room is sprayed with five per cent. solution of carbolic acid for one or two hours immediately before commencing operative work. This casual preliminary one hour's work is the exclusive function of the spray, at time of writing, in Vienna. Every article of furniture is thoroughly washed and cleaned; the asphalt floor, which inclines upon all sides to a hole in its centre for the escape of water, is thoroughly scrubbed under running water before and after operations.

There are held in readiness before each operation towels, napkins, linen compresses, and large linen sheets, soap and brushes, with a razor; also several wash-basins, filled with a two-and-a-half-per-cent. solution of carbolic acid, for cleansing the hands during the operation.

Five other basins are arranged upon a table: the first containing linen compresses; the second, drainage-tubes and patent needles; the third, bits of Bruns's cotton; the fourth, a special sponge for cleansing the field of operation; the fifth, the sponges to be used during the operation.

Two irrigators, which are filled with two-and-a-half-per-cent. solution of carbolic acid immediately before the operation. These irrigators are suspended from the walls, and their contents are conveyed by rubber tubes to the operating-table. A glass vessel containing antiseptic silk. Two well-cleaned, deep dishes, one larger, one smaller. The larger dish is placed upon a small table furnished with rollers,

and is destined to receive all instruments when not in use. The smaller dish holds thread and ligature-materials. Both dishes are filled with two-and-a-half-per-cent. solution of carbolic acid.

Bandages, cotton, iodoform, and carbolized gauze and mackintosh are contained in clean, air-tight receivers.

II.—CLEANSING OF THE HANDS OF THE OPERATOR AND OF THE ASSISTANTS.

The hands and forearms of the operator, and of each assistant who comes in contact either with the wound or an instrument employed in the operation, are washed with soap, and the finger-nails conscientiously brushed. The hands are then dipped in a two-and-a-half-per-cent. solution of carbolic acid, and are not dried. If operator or assistant has been exposed to a special source of infection, such as a vaginal or rectal examination, some substance acting mechanically, such as river-sand or almond flour, is used to remove any secretion still adherent. After this, the hands are dipped into a ten-per-cent. solution of permanganate of potassium. The red stain of this latter agent may be removed by submerging the hands immediately in a ten-per-cent. solution of commercial hydrochloric acid, or a saturated solution of oxalic acid.

III.—PREPARATION OF THE PATIENT AND FIELD OF OPERATION.

Every patient receives at least one warm bath before being operated upon. Immediately before the operation, the field of operation is shaved, and thoroughly scrubbed with brush and soap. All foreign bodies adhering to the skin-surface, such as tar or paint, are removed by oil, turpentine, or sulphuric ether. After this thorough cleansing, the field of operation is irrigated with a two-and-a-half-per-cent. solution of carbolic acid.

To protect the patient from the absorption of carbolic acid, the dependent portions of the body are smeared with vaseline.

The covering of the operating-table is of black caoutchouc.

Around the field of operation are placed black caoutchouc cloths, and upon this basis are arranged linen compresses saturated with a two-and-a-half-per-cent. solution of carbolic acid. Upon these compresses rest instruments during the pauses of the operation.

IV.—PREPARATION OF INSTRUMENTS.

Cutting instruments are frequently polished and sharpened, and are only placed in two-and-a-half-per-cent. solution of carbolic acid immediately before the operation. Blunt, nickeled instruments are not affected by the acid, and may be placed at once in their receivers. Knives soiled with blood or pus are placed first in water, washed with soap and brush, dried with a clean cloth, and rubbed with a piece of soft leather until they are perfectly bright and clean. After use by infectious wounds, blunt instruments are heated to a red heat and then fresh nickeled; cutting instruments are left lying twelve hours in a five-per-cent. solution of carbolic acid, then polished and sharpened.

The ligature and suture-silk, which is contained in closed glass vessels in five-per-cent. carbolic-acid solution, and the wire and lead plates, which are preserved in ten-per-cent. carbolized glycerin, are placed before use in a two-and-a-half-per-cent. solution of carbolic acid.

Salicylic acid may substitute carbolic acid, if the latter is unpleasant to the operator; but its action upon steel is greater.

V.—PROCEDURE DURING THE OPERATION.

The operator and assistant are clad in long, freshly-washed linen coats.

The field of operation is frequently irrigated with a two-and-a-half-per-cent. solution of carbolic acid: if the field of operation is in the vagina or rectum, the mucous membrane is continually irrigated with one-per-cent. carbolic-acid solution.

The sponges are carefully disinfected before use, and are burnt after the operation.

Arrest of hemorrhage is effected with a great degree of nicety; each bleeding point is secured by a silk ligature or by torsion. All operations upon the extremities are performed with the aid of Esmarch's rubber bandage and tube. In operations where other methods of arresting hemorrhage are not available, the wound is irrigated with ice-water, filled with iodoform gauze, and compressed from without. When this procedure is insufficient, the wound may be plugged with Penghawar Djambi—the hair of an East Indian tree-fern—permeated with iodo-

form. Monsel's solution is banished from the clinic.

After the completion of an operation, the wound is irrigated with two-and-a-half-per-cent. carbolic-acid solution, and dried with sponges to remove any blood-coagula or foreign matter.

A foreign surgeon observes with pleasure the extreme care bestowed upon the drainage of wounds in Billroth's clinic. It ought to be remembered in this connection that Billroth has employed drainage-tubes since 1861. Superficial wounds, whose surfaces can be cleansed and approximated by compresses, receive no drainage-tubes, but deeper ones are carefully drained, so as to avoid any accumulation of the first and most dangerous secretion. To this end, the drainage-tubes are inserted into the most dependent portions of the wound, as determined by the patient's posture in bed. To effect this, it is frequently found necessary to make openings by means of the knife or drainage-trocar. After each tube has been demonstrated to be performing its function, by irrigating the wound and observing the promptness with which the fluid is removed, the surfaces of the wound are approximated by two sets of sutures,—one deep, one superficial. The deep sutures are of strong silver wire, and are retained in their places by shot compressed against small lead plates; the superficial sutures are of carbolized silk, No. 4 or No. 5.

VI.—THE DRESSING OF THE WOUND.

Billroth's dressing of the wound differs from Lister's chiefly in the substitution of carbolized gauze by iodoform, in the form of powder or of gauze.

Outside of the operating-room, iodoform is employed:

(*a*) As a powder, sprinkled over wounds, as upon the perineum, by means of Dr. Wölfler's iodoform-duster.

(*b*) As gauze, which may be either (*a'*) dry, "hydrophile," or (*b'*) adhesive. For the preparation of hydrophile iodoform gauze (*a'*), a coarse, unbleached muslin, which has been deprived of its fatty particles, is placed in a basin, washed with carbolic acid, and is sprinkled with iodoform in form of powder until the cloth assumes a yellow color. According to the thoroughness of this operation, the gauze contains from ten to twenty per cent. of iodoform. Fifty grammes of iodoform are sufficient to impregnate six and a half metres of muslin. Hydrophile gauze costs in Vienna about eight cents per metre.

For the preparation of the adhesive iodoform gauze (*b'*), the muslin, deprived of its fatty particles, is saturated with a mixture of alcoholic solution of colophonium and glycerin. The gauze is dried carefully and impregnated with iodoform in the same manner as the hydrophile. For six metres of gauze it requires two hundred and thirty grammes of iodoform and one hundred grammes of colophonium, which is dissolved in twelve hundred grammes of ninety-five per cent. alcohol, to which fifty grammes of glycerin are added. This adhesive iodoform gauze costs in Vienna about thirty-two cents per metre.

The chief function of adhesive iodoform gauze is the arrest of hemorrhage in cases of parenchymatous hemorrhage.

(*c*) As iodoform-glycerin. This preparation consists of ten to twenty parts of iodoform to one hundred parts of glycerin, and is employed for injection into cold abscesses after the evacuation of pus by puncture or incision.

(*d*) As iodoform-collodium. This preparation is composed of iodoform one part to collodium ten parts, and is used in enormous quantities in the ambulatorium. It is a sovereign remedy for cuts and slight bruises. An ethereal solution of iodoform (one part iodoform, seven parts ether) forms a very convenient covering for syphilitic scleroses, and for mucous patches in the buccal cavity.

(*e*) As iodoform-bacilli. The formula for this preparation, as it is found in the pharmacy of the General Hospital, is

Iodoform. pulv., 20.0;
Gummi arabici,
Glycerini,
Amyli, } āā 2;

Fiant bacilli diversi magnitud.

The value of these bacilli cannot be overestimated when fistulous tracts or inaccessible wound-surfaces are to be medicated. In endometritis, cystitis, pyothorax, and certain urethral affections, the iodoform-bacilli are evidently of great worth.

(*f*) As iodoform-vaseline. This salve varies in the amount of the drug contained from twenty to fifty per cent., and is used as an application to venereal ulcers.

THE KERATOSCOPE.

Read before the Philadelphia County Medical Society, May 16, 1883,

BY M. LANDESBERG, M.D.

ALLOW me, Mr. President, to present to you a new instrument, which is of very great use in the daily ophthalmic practice. It is the keratoscope, devised after Javal's suggestions by Dr. A. Placido, of Oporto (Portugal), in order to obtain by simple means, and without much loss of time, a correct view of the topography of the cornea, and to ascertain any difference of curvature between the different corneal meridians, or in the different segments of the same meridian. The instrument is based upon the principle of using the reflex images of the cornea to detect any asymmetry or irregularity of the latter. This principle lies at the base of the ophthalmometer of Helmholtz; but this ingenious instrument is very difficult to manage, and can, therefore, only be used for strictly scientific investigations. The modified ophthalmometer of Coccius is also too complicated to be used in the office-hours of the oculist. Placido's keratoscope supplies the wants of practice. It is simple and handy, and can be used as conveniently as our common ophthalmoscope. It consists of a metallic disk of twenty-three centimetres in diameter, with a circular hole of five millimetres in diameter, and with a handle. The front side shows alternate black and white concentric rings, while the back side presents a plain, black surface, with a small cylinder adjusted to the circular opening.

The examination is made in the following manner. The patient is seated with his back to the window; the physician stands before him, lifting with one hand the upper lid of the eye examined, and holding with the other the instrument before his own eye, through the cylinder of which he looks at the patient's cornea, at a distance of about six inches. The concentric black and white rings on the front side of the instrument are the object, the diminished reflex images of which are projected on the cornea of the patient and are seen by the physician. Care should be taken that the patient should fix the central hole of the disk, and that the latter should be kept perfectly vertical, parallel with the visual line of the observer, as well as with that of the subject under examination.

If we examine in this way a normal cornea, we will obtain the reflex image of perfect circles. Any deviation from the normal curvature of the cornea will show itself by the distortion of these circles. The inference as to the great use of the keratoscope for the diagnosis of astigmatism and keratoconus is obvious. But it is no less valuable a contrivance to ascertain the existence of any increased tension of the eyeball, and to detect any deviation of the latter from the normal position.

The existence of abnormal regular astigmatism is shown by the fact that the circles of the keratoscope appear in the reflex image elongated in the direction of the weakest meridian. They assume the form of ellipses, the short axis of which will correspond to the meridian which has the shortest focal length. After operation for cataract we can see at a glance how the vertical meridian of the cornea has been flattened: the peripheric circles of the disk appear in the reflex image elongated and slightly eccentrical.

In irregular astigmatism the centre of the disk appears in the reflex image eccentrical; the circles are irregular, drawn to one side or to the other, in some parts bent or crooked.

In keratoconus we have, instead of circles, irregular and broken lines; in many instances the centre of the disk appears in the reflex image double.

Hasner was the first to avail himself of the keratoscope in order to test the ocular tension. If, while inspecting a normal cornea with the keratoscope, we press with the tip of a finger upon the eyeball, we will find that the concentric circles will undergo a change: they will become distorted because the cornea has also changed its shape. In glaucomatous conditions of the eyeball, pressure upon the latter, however, will either not produce any change whatever in the shape of the cornea, or but a very imperceptible one. In either instance the configuration of the circles in the reflex image will remain unchanged.

To find out with the keratoscope whether there is any deviation in the equilibrium of the ocular muscles, we proceed as follows. The keratoscope is kept strictly in the meridian line, and we direct the patient to fix straight with both eyes the central hole of the disk. If there be perfect co-ordination of the ocular muscles, the reflex images will be at corresponding points of the two cor-

neæ; but if the equilibrium of the muscles be disturbed, the reflex images will appear on different points of either cornea. In this way we are able to detect, with the greatest accuracy, the slightest lateral deviations of the eyes, particularly minute differences in the height, which may escape our mere ocular inspection. This method is of very great use in instances of paralysis of the ocular muscles.

If, in instances of convergent or divergent strabismus, we cover the fixing eye and the squinting one assumes the fixation, the slightest change in the position of the latter eye is most clearly brought to view by the displacement of the reflex image, which is the inverse of the motion of the squinting eye.

REMARKS ON HYDROPHOBIA.

Read before the Philadelphia County Medical Society, May 23, 1883,

BY CHARLES W. DULLES, M.D.,

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IN that part of his "Medical Inquiries and Observations" in which Dr. Benjamin Rush speaks of hydrophobia, he says, "In entering upon the consideration of this formidable disease I feel myself under an involuntary impression somewhat like that which was produced by the order the King of Syria gave to his captains when he was conducting them to battle: 'Fight not with small or great, save only with the King of Israel.'"

It is with a similar feeling that I come before you this evening. For the more I have studied the subject of hydrophobia the more have I felt the intricacy of the problems it presents for solution, and the difficulty of formulating an opinion in regard to them which shall avoid the mistake, on the one hand, of too great credulity, and, on the other, of too stubborn a scepticism.

This difficulty is so great that I have no idea at this time of attempting to discuss the subject in general, but simply to consider the state of knowledge in regard to it, and to inquire whether this cannot in some way be improved.

More than a year ago I was called one morning to attend a boy who, about a month after being bitten by a dog, had fallen into a state presenting an almost typical picture of what is known as hydrophobia. The course of this case (which I described before the West Philadelphia Medical Society: see *Medical News*, May 27, 1882) was brief and striking. Notwithstanding every effort that could be suggested from my own stock of knowledge, and from the larger one of Dr. C. K. Mills, whom I asked to see the case with me, the young patient passed through an extremely interesting series of phenomena of sensori-motor and ideo-motor excitation, and, within twelve hours after I first saw him, died.

The scenes of this day put to a sharp test my previous convictions in regard to hydrophobia, and left me in a state of uncertainty as to their correctness, which has prompted me to give a large part of my leisure during the last fifteen months to the work of studying what others have thought and written about it.

For those who would study the subject of hydrophobia exhaustively there is a mass of literature whose great extent may be estimated from the fact that in the "Literatura Medica Digesta" of De Ploucquet, published in 1808, there are sixty-four quarto columns of titles on Hydrophobia and Rabies. Besides these, there have been many writings which are not included in this list, and which have appeared since it was collated. A great part of the literature of hydrophobia, though curious and interesting, is but unprofitable reading. Yet happily he who will be satisfied to learn the most salient features of the disorder in a sort of epitome may find excellent accounts of its history and phenomena, and full discussions as to its nature, in certain articles contained in general treatises. Of these I would name the article on "Hydrophobia," by J. L. Bardsley, in Forbes's Cyclopædia of Practical Medicine; that on "Rabies," in Copland's Dictionary of Practical Medicine; that on "Hydrophobia," by Ernest Hart, in Cooper's Dictionary of Practical Surgery; that on "Hundswuth," by Reder, in Pitha and Billroth's Handbuch der Chirurgie; that on "Wuthkrankheit und Wasserscheu," by Virchow, in his Handbuch der Speciellen Pathologie und Therapie; that on "Hydrophobia," by Bollinger, in the translation of Ziemssen's Cyclopædia; and, finally, those on "Rage Animale," by Signol, and on "Rage Humaine," by Doléris, in the recently-issued Nouveau Dictionnaire de Médecine et de Chirurgie. So far as my observation goes,

there is not a single monograph on hydrophobia which can be compared, in interest or in instructiveness, with these articles.

But no one can claim to have gotten at the best that has been said and written about this subject unless he has gone over the papers and discussions upon it read and spoken before the French Académie de Médecine during the last twenty years, where may be found the views of Tardieu, Trolliet, Trousseau, Villermé, Vernois, Raynaud, Lannelongue, Leblanc, Boudin, Decroix, Colin, Pasteur, Galtier, and Bouley, —all names inseparable from the history of hydrophobia. Nor can one be said to have completed even an elementary course of reading unless he has acquainted himself with the writings of Lauder Lindsay on "Madness in Animals," and kindred subjects, in the *Journal of Mental Science* for 1871, 1877, and 1878; as well as with those of Daniel H. Tuke on "The Influence of the Mind upon the Body," in the same magazine for 1870, 1871, and 1872. I would also strongly recommend to every investigator a very valuable series of articles —not referred to by a single writer on hydrophobia—on the Physiology and Pathology of the Saliva, by Dr. Samuel Wright, which were published in the London *Lancet* during 1842 and 1844.

Even this curtailed list may seem a sufficiently long one; but it is, as has been intimated, small compared with the mass from which it has been selected.

A cursory glance over the history of hydrophobia discloses the fact that the first authentic record of its occurrence was made about three hundred and fifty years B.C., by Aristotle, who, singularly enough, says that dogs are subject to *lyssa*, or madness, and that all animals bitten by mad dogs become rabid, except man. After Aristotle there is no authentic allusion to it to be found until the time of Cælius Aurelianus, who gave a good account of its symptoms. About the time of the Christian era it begins to be mentioned pretty regularly by medical writers, Dioscorides, Celsus, Galen, and others speaking of it specifically. After the time of Galen a wider and wider departure from the simple dictum of Aristotle is to be observed. So far from holding man exempt from the communication of rabies even from mad dogs, he came to be considered not only liable to it, but liable from bites of a great variety of rabid animals, such as dogs, cats, wolves, foxes, bears, and even men. At the same time records began to accumulate of hydrophobia acquired in other and most fantastic ways. Thus, the contagion was attributed to eating the flesh or drinking the milk of rabid animals, to breathing the exhalations of a person already affected. It was said to have been conveyed by fomites, by kissing, by coitus; and it was a question whether it might not descend by heredity. It has been credited to the bite of a boy by an angry comrade, and even to a bite inflicted upon himself by a man in a passion; while there are single cases where it has been seriously believed that hydrophobia was caused by the bite of a duck and by the peck of an angry cock. It will not be surprising, after this, to hear that hydrophobia has been attributed to the bites of dogs not rabid, or that it has been believed to have arisen spontaneously in man as well as in the lower animals.

This process of development extended also to the symptomatology of hydrophobia. From a condition fitly expressed by the word "mad," it came to be regarded as one in which there was a simulation of the canine nature. Patients no longer simply fell sick and dreaded attempts to drink water; now they began to act like dogs. They howled and barked and tried to bite. They were said sometimes to turn round and round before lying down, as dogs are often seen to do. Their eyes were said to glow in the dark like those of cats.

The reputed length of the period of incubation of hydrophobia bears the marks of a similar process. Originally we find nothing said about such a period. Aristotle says all animals bitten by a mad dog become mad. The inference is that they do this in a reasonably short time, as they die after being bitten by venomous serpents. But after a while the idea of an incubation comes in, and grows and changes until its length is variously estimated from half a day to twenty—even to forty—years.

Finally, as was natural, while the notions in regard to the causation and the character and order of the manifestations of hydrophobia underwent such change, the methods of treatment multiplied and became more remarkable. Pliny recommended eating the raw liver of the dog that did the biting, or the saliva from under his tongue. Celsus advised throwing the victim of hydrophobia unexpectedly into a pond, when

if he could not swim he was to be allowed to sink, and if he could he was to be held under until he was filled with water. Charms, mummeries, and nostrums—some of the most repulsive character—were constantly employed both to avert the outbreak of hydrophobia and to cure the fully-developed disorder. Bleeding, saturation with mercury, and such medicines as belladonna, opium, prussic acid, and the venom of the viper, were used, not to speak of the heroic method of suffocating the subject of hydrophobia between two feather beds for his own good and for the safety of others.

Thus, under a constantly-accumulating supply of testimony and with an ever-increasing pretension to knowledge, the ignorance of the medical faculty in regard to hydrophobia grew more and more profound. Here and there a writer rejected some egregious absurdity; but the current of misapprehension and error grew stronger and broader until it swept before it even the best intelligences so late as the beginning and middle of the last century. Towards its end protests began to be made and efforts to extricate the subject from the mass of falsehood with which it was encumbered. But how mistaken some of these efforts were may be gathered from the fact that so able a man as the late Dr. Rush regarded hydrophobia as a malignant fever,—though this ought not to be stated without adding that he, with the modesty of true greatness, acknowledged that he might have been misled by the principles of fever he had adopted, and hoped the reader might not be discouraged by his errors from using his reason in medicine.

The present century has supplied almost everything that can be considered of value in forming a correct opinion of the nature of hydrophobia. This is largely due to the advances that have been made in experimental pathology and veterinary science, and more than all to the incomparable researches of both veterinarians and medical men connected with the French Academy of Medicine during the last thirty years. The outcome of these researches has led Doléris to the roseate opinion that our pathological knowledge concerning rabies "is but little behind that which we possess about the majority of diseases of the same order thus far studied."

But it is not necessary to go so far as this to do justice to our own age, for it is not without its own faults of pretension and credulity, and if we examine the state of knowledge in regard to hydrophobia as it is represented by the most recent writers, we find that it is still dark, confusing, and unsatisfactory.

As to its causation, for example, we find that Virchow admits the possibility of contagion by the handling of sabres used to kill rabid dogs. Decroix, so recently as 1863, felt called upon to eat the flesh of a rabid dog, both raw and cooked, to disprove that this could communicate hydrophobia. Vernois, about the same time, said it could be conveyed from man to man; and Bollinger admits the possibility of its communication by the migrations of small animal parasites.

As to its incubation, while Dolan says this varies from one to three months, and that there is a scientific certainty of escape if an outbreak does not occur within a year, Trolliet credits a case where the period was less than a day; Doléris says fifteen months is as long an incubation as is indubitable; Bollinger is credulous up to two years; while Drs. Harrison Allen and H. C. Wood, of this city, had in 1881 a case they considered one of hydrophobia where the inoculating bite occurred three and a half years before; and Sir William Gull (*Lancet*, November 17, 1877, pp. 745, 746) records a case that came under his own observation in which he says the period of incubation was thirteen years.

A consideration of the symptoms attributed in most recent times to hydrophobia discloses no less confusion than exists in regard to its means of communication and period of incubation. There is a certain agreement as to its general character, but there is the greatest possible disagreement as to its details. According to many authors, at its outbreak the site of the inoculation wound manifests changes of an irritative nature. It is said to become painful, to be the starting-point of peculiar sensations or of wandering pains. At times it changes color, reopens, and discharges an ichor. Others, and the great majority of observers, have not seen any such phenomena.

The manifestations of general constitutional disturbance, also, have been different and even contradictory. The striking symptom of dread of water, which gives the disorder its name and which the accounts prove has been relied on as pathognomonic in a multitude of cases, is by

the best authors set down as of little diagnostic value, being absent in many cases of real hydrophobia and present in all or almost all spurious cases, while rabies in the lower animals is marked by a great greed for water. We may also recognize even among modern writers and reporters of cases a singular disposition to attribute to hydrophobia a resemblance to canine characteristics. Barking, biting, and howling like dogs are set down over and over again as symptoms, when their occurrence is in fact rather a strong ground of suspicion that the case is one of spurious hydrophobia. Such a case was reported last year by Decroix, and said to have been cured by pilocarpine, but it was disproved by Bouley. Some writers attach importance to the presence of albumen in the urine. The latest example of this which I have found is in a clinical lecture by Dr. Bristowe, senior physician of St. Thomas's Hospital in London, published in the *British Medical Journal* for April 21 and April 28 of this year (1883). But, in fact, the presence of albumen is inconstant, and its value as an evidence of hydrophobia is impaired by the frequency of its appearance in other convulsive disorders.

Again, some of the most eminent of recent writers speak of satyriasis and nymphomania as phenomena of hydrophobia. These manifestations are not only remarkable, but also of a suspicious significance, when it is observed that they present themselves only within certain geographical limits. Thus, they have been observed in France and Germany, but not in England. Grisolle records a case called hydrophobia where a man practised coitus thirty times in a night, and Reder another where the wife was "stormed," and where there was continual erection of the penis and ejaculations, often with some blood following; while Bardsley says that there occurs no satyriasis or nymphomania in Britain, and I have found no record of its occurrence in America.

About the character of the convulsions of hydrophobia there is substantial agreement among modern observers. They are rarely tetanic, but of an intermittent and clonic kind. Yet there can be little doubt, in reading many of the accounts, that they were caused, or at least aggravated, by the handling patients have received from their attendants. To understand this, we have only to picture to ourselves the mental condition of the victims of this disorder who were treated according to the method of Celsus, or whose neighbors thought it an act of humanity to smother them between feather beds. To a lesser degree such excitements are probably produced in almost every case in consequence of the ignorance and fear of the by-standers. In the case which I had, and which was as near typical as could be imagined, the violences of my patient ceased immediately upon my removing the restraints against which, on my reaching him, I found him furiously struggling.

Let us now turn our thoughts for a moment to the issue of hydrophobia. Most modern authors regard it as inevitably fatal. From time to time cures have been reported. They were commonest in the heyday of those violent methods of treatment which the present generation condemns as cruel, or of those fanciful remedies which it regards as absurd. The use of charms and nostrums claims its cures, and so do ducking, and bleeding, and mercurial saturation, and such violent poisons as belladonna in large doses, and prussic acid, and, last of all,—in our own days,—pilocarpine, oxygen, and curare. How this latter has sometimes been administered may be seen in the lecture of Dr. Bristowe, published only a month ago (*British Medical Journal*, April 21 and 28, 1883), where the lecturer says he gave hypodermically, within twelve hours, doses of curare which, when added together, I find amounted in all to more than *seven and two-thirds grains!* This patient did not recover, however, and the lecturer admits that after its use he learned that the curare had been kept in stock for some time and probably was not of full strength.

But perhaps the most confusing aspect of the present condition of knowledge about hydrophobia is seen when we direct our attention to the teachings in regard to its pathological lesions. In addition to the physical peculiarities said by some observers to be discoverable at the seat of the inoculation wound,—namely, breaking out anew and discharging an ichorous secretion,—a specific form of vesiculation was described by Urban, about the beginning of this century, as occurring just before or at the time of the outbreak of the disorder. And this statement of Urban's has secured a certain number of believers.

Others, even very recently, have looked for and found the *lyssi* under the tongue, described first by the ancients, and again, in the beginning of this century, by Marochetti, an Italian adventurer, who said he had learned from the Cossacks of the Ukraine the secret of their significance, and that their removal eliminated the virus from the system. Others still have found lesions of the fauces, of the salivary glands, of the respiratory and digestive tracts, or of the brain, spinal cord, and nerves, which they hold to be pathognomonic of hydrophobia. But all these have been shown by other observers to be inconstant and unreliable. They are declared to be effects, not causes, and in some cases clear evidence that the disease was something other than hydrophobia. Even such a writer as Bollinger, who contributes the article on hydrophobia in Ziemssen's Cyclopædia, and whose credulity in regard to the communication of hydrophobia has already been alluded to, denies that post-mortem examination of dogs or men has contributed anything of value to our knowledge of the pathogenetic process of rabies or hydrophobia.

It might be expected, of course, that a microscopic germ, lying at the bottom of all the trouble, would be pointed out; and, indeed, Hallier has described one, which he named the *lyssophyton*, the like of which Klebs says he too saw. But the Milan Commission appointed to investigate the subject of hydrophobia soon exploded this error. Not very long after this, Pasteur made some experimental inoculations with the saliva of a patient supposed to have died of hydrophobia in a Paris hospital, and rushed before the public with the announcement of a germ he had discovered peculiar to the disorder induced by inoculation with rabietic saliva. But that this was a premature assertion he himself soon found by experiment, and frankly acknowledged that similar effects to those of the inoculation of his supposed rabietic saliva were produced with the saliva of patients sick of other diseases, and even with that of perfectly healthy individuals, —experiments which have been repeated and confirmed by Sternberg and Formad in this country.

And now let us turn our attention to some of the views in regard to the nature of hydrophobia entertained in modern times.

Dr. Rush says, "The disease produced in the human species by the bite of a rabid animal is a malignant fever."

Dr. Physick thought that in hydrophobia death was due to spasmodic constriction of the glottis, and suggested opening the trachea for its relief.

Copland asks, "Is the secretion merely the vehicle of a nervous aura or emanation, which is actually the infecting agent, and which is retained by its vehicle only for a time?" Again, he says, "The supposition, lately published, that there is no such specific disease as rabies, and that it is merely the result of mental anxiety, etc., is only one of the absurdities thrown up on the surface of medical doctrine, and hardly deserves mention, and much less refutation."

Tardieu rejects the idea that it is a disease of imagination and fear, saying that the commission of which he was a member reported thirty cases under five years of age. He believes, also, in the occasional spontaneous origin of rabies, and gives two cases, one a cat which became rabid from a burn, and the other, also a cat, which became rabid from being robbed of her young.

Jolly, speaking before the Académie de Médecine, turns the tables on the doubters by asking who knows but that hydrophobia acquired by a forgotten bite may have been mistaken for acute meningitis, tetanus, pernicious cerebral fever, acute mania, or other nervous affections? He believes that it may be caused by the bite of a non-rabid dog, and credits a case where a young woman acquired hydrophobia by being bitten by a dog she was suckling,—the dog not being rabid, but only irritated by a correction.

Pasteur, whose acquaintance with rabies is not very great, asserts most positively that he has produced it by inoculation of fragments of the medulla and frontal lobe of the cerebrum and the cephalo-rachidian fluid, and that the incubation is shortened and the communication made surer by inoculating on the surface of the brain, and by using the unadulterated cerebral substance of a mad dog.

Bouley, by far the most careful and thorough student of this subject in France, agrees with Boudin that there does not exist a *scientific* proof of a single case of spontaneous rabies, and illustrates the difficulty of getting at the actual truth about so-called cases. He quotes Renault as

being a most scrupulous observer, and as telling him and Reynal that after thirty years of research there were only three cases of rabies that he dared to consider certain, and that for these he trusted to the fact that he had got the details from persons he considered entirely trustworthy. Bouley, uncertain in regard to the possibility of a spontaneous origin of rabies, illustrates the difficulty of getting at the truth by a capital story, and immediately afterwards furnishes in himself an example of the error to which this difficulty may lead, by saying he would be positive of such an origin if he had some more facts like those given by Leblanc *fils*,—the latter being a history of a case which was subsequently *detectived* by Decroix and shown to be the very opposite of what Leblanc *fils* believed, and, in fact, a clear case of inoculation.

Virchow believes that certain individuals have a predisposition to hydrophobia. He believes in a specific virus, and that it is transmissible by retro-inoculation. He admits the probability of ancient accounts of its transmission by means of weapons used to kill mad dogs with, and by scarifying and blood-letting instruments.

Roucher calls "very probable" a case of communication, by suckling, from a negro woman to her child.

Doléris fully accepts the specific theory of hydrophobia, believing that it is caused by the inoculation of a distinct virus which elects the nerve-centres. He believes the virus resides in a germ, and quotes Nocard as having obtained from saliva, *by dialysis*, a solid substance which, being inoculated, produced "positive results,"—which the liquid parts did not.

Bollinger denies the spontaneous origin of rabies, but intimates that fleas and lice may transfer the poison and thus produce inoculation. He also admits that meat and milk may communicate it, though this is "extremely rare." He says further, "The poison is only in very rare cases communicated in any other way than by the bite of a rabid animal: such cases, for example, are those in which infection takes place by means of *coitus* [!], or through intermediate vehicles, or by the consumption of milk." He considers hydrophobia to be inevitably fatal.

Many other authors might be quoted who, though differing very materially in regard to the details of hydrophobia, have all been believers in its specific nature and inoculability.

But there have not been wanting in modern times those who have been led to other views by observing the discrepancies in the facts and theories of these very believers. Bosquillon, a physician to the Hôtel-Dieu in Paris, in the beginning of this century made a vigorous, though at that time futile, stand against the commonly-received opinions about hydrophobia. He pointed out the unreliability of the evidence upon which the belief in its specific nature rested, and the influence of the imagination in producing morbid physical and psychical conditions. He claimed that the ravages of so-called hydrophobia were increased by the popular education on the subject,—in other words, by the very means taken to warn and defend the community against it.

Girard and J. Simon supported the views of Bosquillon. Not long after, White demonstrated his conviction that hydrophobia was not caused by the inoculation of a virus peculiar to the saliva of a rabid dog, by inoculating a variety of animals, and, finally, himself, with such saliva,—all without injurious results.

Maschka, in Prague, and Lorinser, in Vienna, have denied the specific nature of hydrophobia.

Dr. Burder (in the *British Medical Journal*, October 26, 1872) bases a like opinion upon the incubation, which is without parallel in other diseases, upon the similarity of the phenomena of hydrophobia to those of other nervous disorders, upon the sufficiency of the imagination to excite all the symptoms, upon the tendency to invent a connection between the phenomena and a bite, upon the frequent absence in cases of so-called hydrophobia of sufficient evidence of rabies in the dog which did the biting, and upon the frequent lack of good evidence of any bite at all.

Professor Dick, of Edinburgh, says hydrophobia is not the result of any poison introduced into the system, but merely the melancholy and often fatal result of panic fear and of the disordered state of the imagination.

Professor Macleod, of the University of Glasgow, said,—after a scare in consequence of which eleven hundred and fifty-five dogs had been killed by police regulation,—"There is a large lot of nonsense talked about hydrophobia, and people need not be in the least afraid of it."

Dr. Tuke says, "Among the admitted difficulties attaching to the pathology of this disease, that is surely not the greatest which acknowledges the power of the imagination in combination with fear to excite not only a paroxysm in the course of the disorder, but to originate a group of symptoms by central excitation, which, in a susceptible state of the nervous, closely resemble those of genuine rabies." As an illustration of this, he cites a case from Trolliet, where a man, having been bitten by a dog, had no symptoms until three and a half months afterwards, when a dog one day attacked his horse. In a few days hydrophobia manifested itself, and he died on the third day after.

M. Girard de Cailleux, Inspector-General of the Service of the Insane for the Department of the Seine, has called attention to the similarity of the symptoms, course, duration, termination, and cadaveric lesions of acquired hydrophobia (*rage communiquée*) and of acute febrile delirium, saying that this "establishes an identity of nature worthy of attention."

Dr. Lauder Lindsay says, "Hydrophobia in man is frequently, if not generally, the result of terror, ignorance, prejudice, or superstition, acting on a morbid *imagination* and a susceptible nervous temperament." (*Journal of Mental Science*, July, 1871, p. 185.) Again, he says, "It furnishes one of the best examples that could be adduced of the wonderful influence of the mind over the body, and of morbid mental conditions in the generation of fatal physical disease." (*Journal of Mental Science*, January, 1878.)

Finally, I would quote the opinion of our fellow-member Dr. C. K. Mills, who, after seeing three cases of so-called hydrophobia and making a post-mortem examination of a fourth, has said that, while he would not assert positively that there was no such disease as hydrophobia in man, he was prepared to say that he believed all the cases so called could be explained on some other hypothesis, such as that of lyssophobia, tetanus, chronic or acute nerve-lesions, extravasations, etc. (*Medical News*, May 27, 1882, p. 585.)

We see, then, that the study of hydrophobia, as the subject is presented to us in both ancient and modern writings, is in the highest degree unsatisfactory. Where so many contradictions are found there must be a great deal of error. If, now, we attempt to form an opinion for ourselves, we must, I think, find some way to escape from the influences which have introduced so much confusion in the minds of others. It has sometimes appeared to me that we would be no losers if the whole enormous mass of literature relating to hydrophobia were swept away and an opportunity afforded to begin the investigation of the subject without bias by the careful and critical observation of such cases as may arise from time to time, and with the stern determination to eliminate every particle of evidence which is not of a reliability commensurate with the delicacy and gravity of the case.

A careful study of a large number of recorded cases has convinced me that the source of most of the errors connected with this subject is to be found in a general absence of a proper attitude of criticism, together with a deficient acquaintance with the methods by which the nature of all cases of supposed hydrophobia should be tested. The former want ought for its correction to require no more than that attention should be called to it. The latter requires that medical men should be more familiar with all the conditions under which the phenomena usually attributed to hydrophobia may be produced. Here is a field especially inviting for the alienist, the student of mental and psychical phenomena. The ground has already been broken by Tuke and Lauder Lindsay. Others, it is to be hoped, will carry on the work they have begun.

NOTES OF HOSPITAL PRACTICE.

UNIVERSITY HOSPITAL.

CLINIC OF D. HAYES AGNEW, M.D., PROFESSOR OF SURGERY.

Reported by Louis J. Lautenbach, M.D.

NECROSIS OF THE TIBIA.

THE first case this morning is a man who comes with some disease of the tibia, evidently necrosis. He does not remember receiving an injury in this location: however, a scar can here be discerned.

The tibia is one of the bones which is very frequently the subject of necrosis. It lies subcutaneously, and is very liable to injury. The periosteal trouble induced by an injury may extend to the bone and

result in necrosis. The femur also, although not much exposed, is frequently the subject of necrosis. When necrosis of the femur occurs, and there is no constitutional explanation of the process, then it is probably due to the fact that this bone is surrounded by very strong muscles, which, in contracting during very violent muscular action, raises the periosteum from off the bone, thus occasioning periostitis, or, if the denuded surface be at all large, causes besides a death of the underlying bone from insufficient nourishment.

On the inside of the limb we see a large scar which remains from a former operation. The history is that the disease began in February, 1876, since which time the sinus has never healed up. With a probe entering the sinus we can pass through the external shell of bone and touch a roughened surface. We do this to determine whether the bone is loose or not. If it is, it will be necessary to remove it; but if not, we must be content to wait until separation takes place.

To remove a piece that is loose it would only be necessary to cut through the shell of new bone. In the long run such a case gains most by waiting, particularly as our patient can with comparative comfort follow his business.

The pain in the limb, he tells us, is much worse on going to bed. This is probably due to an accumulation of blood in the deeper vessels, the superficial capillaries being comparatively empty. During the day we all of necessity require the superficial capillaries to be active and full of blood. This condition not existing at night, the blood seeks the deeper vessels, occasioning fulness and thus causing the pain.

LATERAL PHARYNGEAL ABSCESS.

This colored man four weeks ago was attacked with pain and swelling of the throat, with difficulty in swallowing. Later a soft swelling was observed, occupying the lateral wall of the pharynx posterior to the last molar tooth. A swelling corresponding to this was observed on the outside of the neck.

Examining his teeth, at least two are observed to be carious. In these same teeth he had pain two years ago, but has no pain at present. Very frequently swellings in this situation are caused by the difficult eruption of the wisdom tooth, it being necessary to crowd its way where there is but little room. There is nothing of this kind here. There is no history of an injury. The group of glands occupying the sterno-cleido-mastoid region are enlarged. There is observed also a pushing out of the carotid artery, as well as enlargement of the temporal and external jugular veins, evidently showing pressure on the venous trunks.

All growths in this region which involve the glands are suspicious; they are apt to be of a malignant character. We know, however, from the rapid growth in this case that it is not of this nature. Pressing on the wall of the pharynx there is not much resistance,—it is elastic; its contents may be purulent, may be serum, or may be granulation-tissue. When I alternately press on the internal and external surfaces of the swelling, I find that it reacts one on the other, showing a communication between the two.

When there is a reasonable ground for hope that a growth is not malignant, then it is always wise to make an exploratory puncture with the needle. We find here not blood, which is apt to appear when we have a malignant growth, but pus, which tells us that we have here an abscess of the lateral wall of the pharynx. Now depress the tongue, and with a straight bistoury open the abscess from within, and we see here a large accumulation of pus which has flowed out. Without the use of the exploring-needle it would not have been possible to determine positively the nature of the case. The carious teeth, which have probably been the cause of this trouble, should be removed as soon as possible.

GONORRHŒA WITH HYDROCELE.

This man has an enlargement of the scrotum, particularly of the left side. It has been examined by transmitted light and found to be a hydrocele. The swelling does not give him any inconvenience, it being of a moderate size. It is best to let him go for perhaps four or five months without removing the fluid. The operation consists in puncturing, removing the fluid, and, if a radical cure is desired, injecting tincture of iodine into the sac.

Besides this, the man is suffering from specific urethritis. The discharge began three days ago, and the inflammation has

extended over the head of the penis and prepuce (balanitis).

We will order him to inject sulphate of zinc, a grain to the ounce, three times a day, it being retained for about five minutes, while the under surface of the penis is being gently rubbed by the hand. In two days he is to use a two-grain solution, and in four or five days a three-grain. If this does not smart, he can go up to four or five grains. Under this treatment the discharge will stop in about ten days.

There are various internal remedies used in such cases. Here we will use capsules of balsam of copaiba, each containing ten minims, four or five times a day. The administration of this remedy facilitates the cure of the disease.

In order to avoid the expense and worry of the frequent journey to the apothecary, I am in the habit of ordering the strong solution at once, and letting the patient dilute it to the requisite amount. For instance, I prescribe—

℞ Zinci sulphatis, gr. vi,
Aquæ, f℥i.

M.—Ft. solutio,

and order him to use it at first in the proportion of one part of the solution to four or five parts of water.

UNUNITED FRACTURE OF THE FEMUR.

This man, aged 50, came to us this morning with the history that three years ago he fell on the right hip, and in consequence of the injury he has not been able to work since.

Looking at the limb, we see that the right one is a little atrophied: this can be accounted for by the rest to which it has been subjected. Placing the two limbs together, we see that the right is the shorter. Measuring from the anterior superior spinous process to the lower edge of the internal malleolus, the difference is found to be one and a half inches. On attempting to turn the foot of the sound limb outward, there is a good deal of resistance; scarcely any is felt on endeavoring to do the same with the other limb. Measuring the distance from the trochanter major to the anterior superior spinous process, there is a difference of one inch, it being so much shorter on the injured side.

Eliminating luxation, but two explanations are possible,—one that there is a fracture of the neck, the other that it is a case of absorption of the neck.

Testing the mobility, I find here a crepitation which I take to be the crepitation of an ununited fracture. However, we must be very cautious at this length of time in saying this positively, as necessarily the ends of the bones must be very much rounded off. When he walks, the trochanter slips up as soon as he puts his weight upon it. He walks very badly, the trouble being, probably, that he has not tried enough. (Patient sent out.)

The trouble with such patients is that they come here for an opinion, and as soon as they have obtained this they go and see their doctor. This fracture occurred at 47 years of age, a little under the time when intra-capsular fractures are liable to occur, although there are cases where intra-capsular fractures have occurred at an earlier period than this.

NECROSIS OF THE FEMUR.

This lad was sent to the hospital with, as you see, evidences of considerable disease of the thigh. It is very much enlarged, and there is a sinus which I doubt not leads down to diseased bone. There is, besides, distortion and considerable shortening of the limb.

This condition might possibly have arisen from hip-joint disease, but it is not due to this cause, as the limb can be well flexed on the pelvis. Besides this negative evidence, the marked enlargement points to necrosis of the femur. The inflammatory infiltration of the soft parts also points to necrosis. Using now the probe, the diseased bone is easily felt. The scars on the thigh make it appear that the actual cautery has been used to subdue the inflammatory disease of the bone. So far as my experience goes, it is of no use whatever. This case is at a part of its course where the best treatment is to remove the necrosed bone. In order to do this, we must first admit him to the hospital.

FOREIGN BODY SUPPOSED TO BE IN THE RESPIRATORY PASSAGES.

This girl comes here with the impression that there is a breast-pin in her throat. Often operations have been performed in just such cases, and no foreign body found. It is always best to see the foreign body before performing an operation.

The breast-pin was an old-fashioned cuff-button, to which a pin one and one-half inches long had been soldered. This she

swallowed three years ago, and six weeks afterwards she was operated on and no breast-pin found.

Some moist râles are found over the girl's chest, but they are not such as we would expect from such a large body. In such cases there is a profuse discharge of mucus in the air-passages, and very moist râles are present; then, also, there are violent paroxysms of coughing, with attacks of impending suffocation. The suffocative paroxysms are not so bad when the body is large as when the body is small. The small body slipping up and down with each act of respiration occasions a reflex irritability by rubbing against the walls of the larynx and perhaps touching the vocal cords.

There have been no violent spasms or suffocative attacks at any time. Probably the body has gone along the alimentary canal, and not into the air-passages.

TRANSLATIONS.

Charcot and Pitres on Cerebral Localization.—From a *résumé* of forty-four cases of various lesions of the cerebral cortex, Messrs. Charcot and Pitres conclude that they prove that there exist certain regions of the brain the destruction of which is not followed by any permanent impairment of voluntary motion. Isolated lesions of the pre-frontal lobes, of the parietal lobes, of the sphenoidal lobes, or of the occipital lobes do not of themselves, necessarily, give rise to permanent motor paralysis. The cerebral convolutions, the integrity of which is necessary to the production of voluntary movements in man, are the ascending frontal and parietal gyri and the paracentral lobule. However extensive a lesion of the cortex may be, if it do not directly or by compression or irritation in the vicinity affect the convolutions just named, they do not give rise to motor troubles.

In a recent work on "Localization of the Cerebral Functions in Man," M. Exner broaches the view that the extent of the area of the so-called latent zone is not equal in the two hemispheres. According to this author, the motor zone should be more extended in the left than in the right hemisphere, and the latent zone is therefore larger in the right than in the left. The cases just summarized do not warrant this opinion: according to Messrs. Charcot and Pitres, the non-motor areas in the two hemispheres should be considered as symmetrical. In each side the non-motor zone comprises all the convolutions except the two ascending convolutions and the paracentral lobules.—*Revue de Médecine.*

Total Extirpation of Carcinomatous Uterus.—Sänger, of Leipsic, reports (*Sep. Abdrück Archiv f. Gyn.*, B. xxi. H. 1) two cases of successful extirpation of the uterus through the vagina for malignant disease. The first patient, 51 years of age, suffered with ulcerative carcinoma of the cervix uteri; the operation of Czerny and Schröder was performed, and was followed by considerable hemorrhage, which continued after ligature *en masse*, and only ceased after the application of a tampon. Four months afterwards, cancer returned, and in the course of a few months later an intestino-vaginal fistula developed, and the patient finally perished of exhaustion, ten and a half months after the operation. The second case was 36 years of age, and had a commencing cancer of the cervix. The uterus and both ovaries were removed, and the patient made a good recovery. Six months after the operation the patient appeared entirely well, with her general health greatly improved and no sign of a return of the disease.

Anatomical Lesions of Myxœdema.—In a well-marked case of a man, 36 years old, who had suffered with myxœdema, Dr. Hemot found the nerve-trunks in the neighborhood (vagus, glosso-pharyngeus, plexus brachialis) very materially thickened. The most marked alterations, however, occurred in the brain and sympathetic. A tumor the size of a small hen's-egg was found at the base of the brain, flattening the optic chiasm. The main cord of the sympathetic, and all its glands and efferent nerves, were much hypertrophied, especially in the cervical region; the semilunar ganglion and solar plexus were also enlarged. Microscopic examination showed true hypertrophy of the normal elements (Virchow's hyperplasia). Four autopsies of myxœdema have been published, but this is the first which showed alterations in the nervous system: it remains to be seen if the very marked anatomical changes have any connection with the disease.—*Gazette des Hôpitaux*, No. 23.

THE PATHOLOGY OF LUPUS.—In a communication to the *Berliner Klinische Wochenschrift* (No. 19), Drs. Pagenstecher and Pfeiffer report that secretions taken from the conjunctival sac of patients suffering with lupus, when injected into the anterior chamber of the eye of rabbits, produced an artificial disease of the iris, containing elements which were recognized as tubercle bacilli. The authors conclude that "if Koch's bacillus is actually the specific sign of tuberculosis, which we do not doubt, the proof is here brought forward that lupus is to be regarded as a localized tuberculosis." This coincides with the view of Demme, quoted previously. — *Deutsche Medizin. Zeitung*, No. 22.

ARREST OF DEVELOPMENT OF ONE SIDE OF THE BODY.—At the meeting of the Berliner Medizinischen Gesellschaft, Dr. Mengel presented a case of hemi-atrophy of the face in a girl 23 years of age, which disparity gradually occurred during a period of five years, from the seventh to the twelfth year of age. The left side of the face was perceptibly smaller than the right, and the corresponding hand and foot were also smaller than their mates. Dr. Mengel had only been able to discover two similar cases, as it differs from most of the cases of hemi-atrophia facialis in the accompanying atrophy in the hand and foot.

AMYLIC ALCOHOL IN WINE.—From an examination of samples of natural wine of Bordeaux and Alsace, M. Henninger detected, besides the ethylic, a considerable proportion of other alcohols, especially of amylic, propylic, butylic, etc. The results of a number of analyses demonstrated that natural wine contains a notable proportion of amylic alcohol, or fusel oil, which is highly poisonous, and which is also found in the spirits distilled from grains, in gin, and in some kinds of brandy.—*Le Progrès Médical*, No. 22.

THE THERAPEUTICS OF EPILEPSY.—Two cases of marked improvement of epilepsy after the administration of atropine are reported by Dr. Max Weiss, who declares that this agent is free from the disagreeable effects of the bromine preparations, and is efficient, in small doses, in lessening the force of the paroxysms and increasing the interval between the attacks. It is useful in all forms of epilepsy. After prolonged administration a decided tolerance is established, so that the dose may be materially increased without causing toxic symptoms.—*Centralblatt für die Ges. Therapie.*

DEATH FOLLOWING EXTRACTION OF A TOOTH.—Two cases of death after extraction of teeth, from infection of the wound, are reported in *Wratsch.* Both were strong, healthy men. The reporter, Dr. Sacharewitsch, recommends the careful disinfection of instruments and hands, and the use of antiseptic washes (carbolized water one to two per cent.), after extraction of teeth. After bleeding has stopped, the wound may be packed with iodoform, and sealed with cotton-wadding dipped in collodion. The pain ceased almost at once, and no reaction occurred, in nineteen cases so treated.

THE FRUIT OF MUCUNA PRURIENS.—Dr. S. Martin calls attention to the fruit of Mucuna pruriens (D. C.), which, being rich in tannin, may be used in medicine, and might become an article of commerce. It is abundant in South America, where its infusion is popularly used as an astringent and sedative for hemorrhoids, and its pulp is applied as a dressing to wounds.—*Bull. Gén. de Thérapeutique*, No. 2.

FATAL ŒDEMA OF THE GLOTTIS IN TYPHOID FEVER.—Dr. Merklein reports a case of tracheotomy for œdema of the glottis in the course of typhoid fever in a woman, 24 years of age. Previous to this complication the nares had been plugged on account of epistaxis. Probably mouth-breathing may have induced this condition of the larynx. Death followed the operation, due to hemorrhage from the air-passages. —*La France Médicale*, No. 69.

BOWDITCH'S FORMULA FOR IRREGULAR HEART.—In a discussion upon heart-disease before the Boston Society for Medical Improvement, Prof. Bowditch said that he had found the following formula of great service in relieving even the most serious cardiac affections. He had used it for the last twenty-five years. ℞ Pulv. digitalis, gr. x; pulv. colchici sem., gr. xx; sodii bicarbonatis, gr. xxx; M. et div. in pil. no. xx. These are to be taken three or four times daily at first; subsequently to be reduced until only one is taken at bedtime; the treatment to be continued for three to nine months.—*Boston Medical and Surgical Journal.*

PHILADELPHIA
MEDICAL TIMES.

PHILADELPHIA, AUGUST 11, 1883.

EDITORIAL.

NEW SCHOOL, OR NO SCHOOL.

THE public seems very much at a loss for an adjective or descriptive title that will serve to classify modern physicians who belong to no school, who subscribe to no therapeutic creed, and who acknowledge the dictum of no medical authority in the treatment of disease. It is evident that a large number of such practitioners now exist,—practitioners who represent the progressive spirit of modern medical science, who are intelligent, earnest, skilful, and successful. The term "old school" certainly does not apply to them, because comparison of their practice with that of physicians of only a few generations ago shows it to be new, and, further, because they are openly and avowedly free from all schools, following the study and practice of medicine as one of the natural sciences, and not according to any exclusive dogma or doctrine. The adjective "allopathic," invented as a term of reproach to distinguish those who did not subscribe to the absurd tenets of Hahnemannism from those who bound themselves by its doctrines, has had a longer lease of life than its merits entitled it to. It is a word of low parentage and bad associations, is untruthful, and should never have been even tacitly accepted by the profession. Hippocrates having declared that "some diseases are cured by similars, and others by contraries," the founder of homœopathy declared that physicians who did not adopt the law (?) of similars (homœopathy) must practise under the supposed law of contraries (allopathy), against which conclusion a verdict of "not proven" must be rendered. Allopathic practice—if there ever existed a sufficient number of practitioners possessing these restricted views to warrant the statement that there was such a practice—would be even more limited and intolerant than homœopathy; but it is safe to conclude that a sect to which the term allopathic could be properly applied never existed outside the imagination of the inventor of infinitesimals and the discoverer of "psora."

Indifference on the part of the regular profession, and ignorance on the part of the public, have conspired to prolong unwarrantably the existence of a word which, being a term of reproach, would be insulting to a scientific physician were it not so completely inapplicable. It seems strange that it is necessary at this late day to point out the fact that there is no such thing in existence as allopathic practice, allopathic medicine, or an allopathic doctor, and to reiterate the truth that the adjective, if it has any meaning at all, implies a restricted practice of medicine in conformity with a supposed law or dogma which has no real existence in science. The apathy (a singularly appropriate expression) of the profession has now tolerated the adjective allopathic for too long a period, and it is high time that it should be generally repudiated. No regular practitioner, without doing violence either to his intelligence or to his self-respect, can permit this inappropriate and insulting term to be applied to himself or his colleagues unchallenged. Apothecaries especially need to bear in mind the real import of this term, and that it is not applicable to the medical profession: with their aid the public may be enabled better to appreciate these facts.

A movement in this direction has already been inaugurated, and physicians in different parts of the country are beginning to protest against being classified as "allopathic" in directories, lists of registration, and other public documents. A timely resolution, advocating more general action

in this direction, was offered by Dr. Charles Denison, of Colorado, at a recent meeting of the American Medical Association, which is still held under consideration by the Judicial Council.

It seems necessary thus to reaffirm principles from time to time, in order that the public, from which our ranks are recruited, may have a clear and just conception of the attitude of modern medicine towards all restricted methods of practice, including schools, 'pathies, and 'isms, which, as we believe, belong exclusively to a bygone age in the evolution of medical science. Some years ago, when the propriety of introducing homœopathy into the Medical School of Naples was advocated, the following reply was made to the petition: "The University of Naples is not a proper field for instruction in homœopathy, because the rational medicine which is imparted here, on the basis of the natural sciences, excludes allopathy as well as homœopathy or any other absolute system or dogma. The study of rational medicine is as far removed from the ancient allopathy, with its blood-letting and purgation, as from the recent delusion of homœopathy, with its ridiculous infinitesimal doses and similia similibus medication."

If an answer to the question of "What school?" is insisted upon, let the practitioner of modern medicine boldly and unhesitatingly reply, "The New School," a school based upon physiology, in which disease is regarded as disordered function, to which morbid anatomy furnishes the key, and whose therapeutics, untrammelled by theory, is founded upon observation and reason. As stated by Prof. Dunglison, "every judicious physician must be an eclectic;" that is, he is not only at liberty to examine all professed improvements and advances in medical science, but it is his duty to give to his patients the benefit of his investigations. This spirit characterizes the scientific practitioner, and makes him independent in his researches, unrestricted in his practice, and non-dogmatic, unsectarian, and liberal in all his relations to his professional brethren and his intercourse with the public. Must he also affiliate with a horde of ignorant and impudent charlatans that practise upon the confidence and credulity of patients? By no means. While affording to others the same facilities that he himself enjoys in the study of medicine as a branch of science, he cannot recognize as physicians those who refuse to use such facilities, nor can he meet upon any common ground practitioners of exclusive systems of medicine, or consult with them without treating them with a degree of consideration which they do not deserve, and acknowledging them, *ipso facto*, as entitled to an equal amount of consideration by the public.

THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION.

WE welcome to our exchange-list the new *Journal of the American Medical Association*, of which we have received the first issues. It is a weekly, of large size, printed on white paper, and covers thirty-two pages, exclusive of advertisements. The *British Medical Journal*, the publication of the British Medical Association, has, we fear, a substantial advantage over our Association journal, judging from the specimens before us,—an advantage which is only imperfectly expressed by its seniority: indeed, it seems unfortunate to the young journalistic experiment that it openly adopted such an elevated exemplar, or that, having done so, it did not copy it more closely. Barring typographical errors, which apparently do not show much tendency to decrease with successive issues (and among which we observe the name of the venerable editor spelled with a capital F in the middle), and the evidences of the existence of some doubt in the editor's mind as to whether it is

the organ of an association or a society, it may on the whole be considered a very creditable piece of amateur journalism, upon which we defer criticism until the first volume is completed. From its pages we may possibly be able, at some future time, to make a few extracts for our readers.

CORRESPONDENCE.

LONDON LETTER.

THE medical profession is still eagerly awaiting the passing of the proposed Medical Bill; but the prospects of its passing are as remote as ever, I fear. Hope deferred maketh the heart sick: certainly then the medical profession ought to be sick indeed. The *ignis-fatuus* seems ever about to be, yet never is.

> "For aye since I could climb a hill
> We have been bothered with that bill.
> Ruin awaited the denial:
> 'Tis fair and just to make the trial."

The limited time which has now to elapse before the houses retire into the country to shoot grouse, share in picnics, be patrons of agricultural and horticultural shows, harangue their constituents, and point out how much time has been deliberately wasted, will, it is to be feared, be far too short for the consideration of such an entirely subordinate matter as a bill affecting the medical profession. Indeed, the present is scarcely the form of government to do anything for our profession, or, indeed, anything with which the art of medicine is concerned. It contains a large radical element; and that element has within it a large section who are homœopaths, or any 'path opposed to orthodox medicine; who disbelieve in the protective effects of vaccination, and have raised one special debate on the subject this session; who have disannulled the Contagious Diseases Act (Human), despite its excellent effects in our garrison towns; and who contemplate an act relating to the immorality of young girls, which carries within it unlimited facilities for extortion.

Up to this upgrowth of radicalism the legislature drew the line at puberty, and distinguished betwixt a child, an infant in the law, and a young woman. The first was supposed to have no rights on her person, and consent did not lessen the criminality of connection with her; the latter was credited with the charge of her own person. Now it is proposed to raise the age at which consent goes for nothing to sixteen; from which no end of difficulties will arise. It is no uncommon matter for girls to be married at, or even before, sixteen; and we might see a man who has married a young girl, and gained the church's sanction thereto, had up on a criminal charge for carrying out his marital duties, without which the marriage cannot be consummated. The radical party has determined that man shall be encouraged to be moral by every probability of disease dogging his errors of the flesh. Now female chastity has to be protected by an act which will soon involve another to protect men from charges levied with the intent to extort money.

Indeed, our legislature is drifting towards the utterest excesses of folly in its attempts to foster morality by edicts. But it would seem they must learn their own folly by such acts as will teach them a lesson, the only thing that ever will; and how many lessons they may require it is not possible to say. At the same time the threatened corporations have been busy in their own interests. Consequently, years after I have disappeared from professional life and gone over to the majority, some future correspondent may quote from a recent letter of mine in relation to some proposed medical bill in the next century, just as in it I quoted from the *Lancet* of over a generation ago. We were confidently told that if the Medical Bill did not pass this session it would have no opportunity of being brought forward again this century, in order to terrify any meditated opposition; but the threat has failed in its object, for opposition is alive, active, and likely to be successful.

But if the medical profession either does not know its own mind, or does not possess the influence with our legislators to get its wishes attended to, it has not been without some honor. The annual conversazione of the ancient Medical Society usually comes off in May, but this year it was delayed until the extension and improvement of the Society's premises had been concluded. Sir Joseph Fayrer is the present President. He was in medical charge of the Prince of Wales when His Royal Highness made his recent tour through Hindostan; and the prince, in remembrance thereof, attended the conversazione to do honor to his physician. Of course the announcement of this drew a large attendance, Sir Joseph himself being popular, and deservedly popular, with the profession. Besides the Prince of Wales, Prince Lucien Bonaparte and other illustrious individuals were present. So the Medical Society has nothing to complain of as regards royal attention. Two days afterwards, the Royal College of Physicians held its annual conversazione; and again the medical profession was fluttered as well as flattered by the presence of royalty. This time H. R. H. the Duke of Albany was received and shown round the premises in Pall Mall by the President, Sir William Jenner, K.C.B., and the Censors of the College. Further, three men of title were present, and I suppose the College felt itself on familiar terms with the aristocracy.

It is usual for the President of the College of Physicians to be made a baronet; but, as Sir William Jenner is that already, it has been decided that an honor shall be conferred on some one else. So it is announced that the honor of knighthood will be conferred on Dr. Pitman, Registrar to the College of Physicians, in recognition of his long and faithful services to that important body.

It is not always clear what constitutes a qualification for knighthood; and the Registrar of the College of Physicians may have quite as strong a claim as the Chief Justice of Fiji, or any other of the persons recently knighted in the shower of such honors which has fallen of late. What his services to the College have been, beyond being a very urbane, gentlemanly, punctual registrar, is only known to a body whose rulers sit *in camera*. What he has done professionally since he was Lecturer on the Practice of Medicine at St. George's Hospital Medical School, or what he may have contributed to medical literature, is unknown to me, and does not appear in the Directory.

A dentist is to be knighted! Mr. Edwin Saunders, F.R.C.S., for many years dentist to the royal family, is to receive this honor. Mr. Saunders has long held a very prominent position in the world of dentistry, and is a worthy recipient of such an honor.

It is unfortunate that in this country there is no purely civil order for distinguished services. A knighthood is now too common as a reward for party political services to carry with it much dignity. Whatever honors are going about, it is well that Medicine have a share, even if it be only among the crumbs which fall from the rich man's table and among the humblest honors. The sister professions, the Church and the Law, come off very differently: the Church has its spiritual peers (a doomed body, it would appear); while the judgeships carry titles usually. But the affairs of the profession are managed in a curious fashion, as the medical press keeps constantly assuring us, while no one professes disbelief! As to the Royal College of Physicians, the said press is in the habit of stating that its actions are inscrutable, and that all hopes of better things must be abandoned. The membership is gained by examination, and is indispensable to holding a respectable hospital appointment; it is the one portal, indeed, through which graduates of universities, or others, non-graduates but ambitious, must enter, and pass on their way to the higher walks of consulting practice. But the Fellowship consists, as a censor once remarked, "in getting eleven of the fifteen voters to look favorably upon your claims." What has to be done to get the good opinion of the said fourteen men is a deep mystery. It is certain a member must *not* canvass; by what occult means, then, these fifteen electors are and have to be reached is unknown to me at least,—as no eleven have ever seen any merit in me. Yet so many men are passed forward to the Fellowship every year; and, though some of them are comparatively unknown to the outside world, it is certain they have merits recognized by eleven out of the fifteen electors inside the council. Hard words have been used about these selections or elections; but self-electing oligarchies are rarely impressed with any opinion but their own. The whole matter has long been a scandal. The hardship of the matter lies in this. The Fellowship is withheld from men who have been guilty of unprofessional conduct; that is a notorious fact. Consequently, when a man has been years a member without being raised to the rank of Fellow, he is open to the suspicion of having been guilty of unprofessional conduct, of which the College thus takes cognizance. Consequently, chief sinners and the merely friendless are mixed together without distinction of persons. Probably no one who has any position of his own cares whether he becomes a Fellow or not. His patients do not ask the question, or care a button. But the lecturers to the College, and, as a rule, its examiners, except in surgery, are chosen from the Fellows. It is always a matter of time for a member to be a Fellow, unless he belong to a teaching hospital. It is easy to see that by this last, and by the matter of examiners, the College rules medical education; and its influence is felt, and unmistakably; but whether it is a happy influence or not, is open to question.

Many suggestions have been made to improve matters. Some have demanded examination; and if this test were adopted there are good grounds for believing that some would soon be Fellows who are not so ranked at present. Another suggestion is that the members should every year elect so many of the most illustrious of themselves to the higher rank. This might be all very well, but the present ruling oligarchy are not in the least likely to abdicate, or hand over their power to any one else. If such change has to be made, it must be done by force, by violence, and the matter is not one ever likely to become a burning question. Many honored names in the past have been without the Fellowship, and it is possible to hope that posterity may have some little respect for men like James Henry Bennett, for instance, who have not succeeded in gaining the good will of eleven out of fifteen voters.

The whole thing is ridiculous, and not far from contemptible; but the open wailings of some members at their exclusion probably induce some of the leading spirits in the council to believe that they wield supreme powers. If these wronged individuals would treat the matter as it deserves, and take no notice of the elections, it would be a great deal better in every way. It would be more dignified as regards themselves; while the littleness that obtains in the secret conclaves would become

apparent to the persons who, shrouded in secrecy, strive to stab those of whom they do not approve. Do they imagine that any right-minded person would value their approval? Persons whose approval is esteemed are usually persons endowed with self-respect. And because an individual happens to occupy a position where he can manifest malice with impunity, it does not by any means follow that every one has to value his opinion. Yet it would seem that this is the view which obtains in the venerable institution in Pall Mall, who is like an aged female in more respects than this one of antiquity. The matter has been looked at as a scandal far away back for many years, and will go on, doubtless, into the far future.

In a leader for January 18, 1840, the *Lancet* bewails the governing bodies of the medical corporations thus:—"They carry on their proceedings in secret. They expend the money of the members," and so on. At that time Sir James Clarke had declined the offer of the Fellowship. Marshall Hall, as only a licentiate, was grumbling that the College library was closed against him. Professor Grant was not even a licentiate; no more was Sir Robert Carswell. The complaint was then of "self-elective councils," to whom the members are "ciphers, if not aliens." "Talent is idle; industry is vain. The stimulus which stirs generous minds in their career is withheld." But just as the remarks about a medical bill held at that time are re-echoed now, so the expressions used about the College of Physicians then may be repeated now, without any change but the flight of time.

It is not the ten men who might be willing to act straightforwardly upon whom it would be well to let in the light of day: it is the five men who can unite to act otherwise, and who are sheltered by the secret conclave, who need dragging into daylight. That this could be done, if any member felt sufficiently keenly and would take the trouble, is probable enough. But it is equally and even more probable that no one will ever be at the pains to compel the council to make public its deliberations.

It is well enough known in life that persons do not desire to keep their actions in the shade if they will bear daylight; indeed, they are only too proud to parade them and get credit for them. When there is studious concealment with outside discontent, and this goes on for years, the inference that there is something which calls for concealment does not need drawing, it suggests itself! The medical corporations are spoken of with disrespect, and charged with the vilest selfishness; yet years roll past after years and things continue the same. Each new President is regarded as the inauguration of a new *régime*, yet the prospect remains unchanged. The present President is the universally-respected head of our profession, loyally and cheerfully acknowledged as such by all. It was thought great changes would follow his Presidentship; but they seem to be still in the womb of the future.

Some malign influence is in action which is hostile to reform in the medical corporations. They clamored to have the proceedings held *in camera;* and any one with a slight experience of the world knows what that means! It means that for once A can do some real good by venturing to say about B what ought to be said and yet he may not prudently say it openly, twenty times C can malign D, and does it with distinct calculable results, without D having an opportunity of dragging him to light. The necessity for secrecy is, after all, the argument of men who have not the courage of their convictions,—who, if they feel something ought to be done, will only do it when it is not to be inconvenient to themselves. And under this pretext a number of other men shelter themselves in private backbiting, who, if their individual iniquities are not known, as a body their conduct is known. And consequently the profession of medicine labors along under a rule which it despises, knowing that certain men cannot be thrown out of the saddle; while the outer world looks on at it in scorn. Fortunately, a man can do good work, if such work is in him, without recognition by a far from impartial oligarchy; while if the world can find a trustworthy medical man who treats his patients honestly, it does not bother its head as to whether he is honored by his college or not; though of course there are persons, not wholly disinterested perhaps, who would wish the world to note such matters.

If the refusal of the Fellowship of the College of Physicians were a direct tangible injury to any member or members, it would not be long before the matter would be taken up in earnest. But, as it happens, it is only a sentimental grievance, to be grumbled over by those who choose to feel aggrieved. The membership is essential, and, therefore, to be secured by examination; but the Fellowship is only the road to certain honors and emoluments, and, therefore, is a matter of favoritism. If a man is not a favorite, all that he has to do is seek honors and emoluments elsewhere; and it is much better for him to turn his energies in other directions, instead of doing nothing but whining that he is passed over, and thus gratifying the malice of those who wish to put a slight on him.

J. MILNER FOTHERGILL.

CHLORATE OF POTASSIUM IN ULCERATING EPITHELIOMATA.—In fine powder this is said to yield excellent results when dusted over the surface of ulcers and ulcerating epitheliomata. The surface should be cleansed and the powder dusted thickly on twice a day. This, it is claimed, relieves pain and promotes healing.—*Canada Lancet.*

PROCEEDINGS OF SOCIETIES.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

A CONVERSATIONAL meeting of the Society was held at the hall of the Society on May 16, 1883. Dr. G. W. Vogler read the following:

CLINICAL NOTES OF A CASE OF ECCENTRIC HYPERTROPHY AND DISEASE OF THE VALVES OF THE HEART, THE VISCUS WEIGHING TWENTY-SIX OUNCES.

W. H. P., aged 41, wagon-builder by occupation, of a delicate frame, under five feet in height, and weighing ninety-six and one-half pounds, was admitted to the German Hospital, December 29, 1882. His chief symptoms were severe dry cough, great difficulty in breathing, excessive over-action of the heart, a pinched and pained expression of the countenance, and dropsy of the feet and legs. All the minor symptoms consequent upon extensive organic disease were also present.

Auscultation and percussion of the chest revealed emphysema of the lungs, great increase of the dulness-area over the pericardial region, the apex-impulse below the sixth intercostal space, very tumultuous action of the heart, and a distinct and loud blowing murmur (the most marked I ever listened to) heard over both aortic and mitral valvular region, especially prominent at the former.

These physical signs so prominently characteristic rendered the diagnosis easy. His urine showed a trace of albumen, which soon disappeared, however. Briefly, his history showed him to have led a hard life, having endured great hardships during the late Rebellion, addicted to drinking, and having had frequent attacks of rheumatism. No history of syphilis.

The treatment adopted was simple,—viz., infusion of digitalis, perfect rest, tonics, and the best diet the house afforded.

His improvement was wonderfully rapid, his weight being one hundred and sixteen pounds when he left the hospital, February 26, 1883. He probably must have abused himself badly immediately after his discharge, because, upon application of his friends, he was readmitted on March 13 with his previous symptoms very much exaggerated, and almost in a state of collapse, with delirious mutterings, face anxious and covered with cold sweat, extremities cold, great difficulty in breathing, and, in short, every evidence of rapid cardiac failure.

Cardiac stimulants and other measures proved of no avail, and death occurred seven hours after admission.

Post-mortem examination proved the heart (specimen shown) of great interest pathologically. Its weight is *over twenty-six ounces*, or, putting it in other words, if about nine ounces be allowed as a fair weight for a normal heart in a subject of the height and weight of my patient, it would be almost *three times* the weight of the healthy organ. For evident reasons, these enormous-sized hearts, in which dilatation extends the area and hypertrophy the thickness of the cardiac walls, are well named "bovine hearts."

The specimen shows great thickening and hardening of the aortic valves through atheromatous and calcareous degeneration, and consequently *incompetency* must have been very marked during life, causing both obstruction and regurgitation, and producing the extensive "eccentric" hypertrophy (hypertrophy with dilatation) noticeable in the left ventricle. So marked is this that a normal-sized heart may be placed within its cavity. This hypertrophy is confined alone to the left ventricle, and is uniform throughout. Atheromatous patches and calcareous points are noticed above, upon, and below the aortic valves. The left auriculo-ventricular orifice is much expanded, and owing to the thinness and distention of the mitral valve, together with the increase of size of the papillary muscles and the chordæ tendineæ, its usefulness was greatly impaired, thereby allowing regurgitation and accounting chiefly for the enormous dilatation of the left auricle. The right side of the heart shows extensive dilatation. The thinness of the walls of the auricles is worthy of notice. The cavities of the left and right ventricles and of the right auricle are laid open. The walls of the left auricle I have left intact, to better give an idea of its size or dimensions by introducing the finger within its cavity through the abnormally large auriculo-ventricular aperture.

The specimen beautifully shows the minute anatomy of the heart and the relations of the parts one to the other. The musculi papillares and the slender chordæ tendineæ, which greatly aid in controlling the mitral valve, are very conspicuous. This is also the case with the columnæ carneæ and the auricular appendage and its musculi pectinati.

I believe the largest heart on record is mentioned by Dr. Bristowe* in his "Pathological Transactions," in a case of aortic valvular incompetency, with probably regurgitation through the left auriculo-ventricular aperture from maladjustment of the valves. This specimen weighed forty-six and one-half ounces avoirdupois, though the subject of the disease was only twenty-one years of age.

Dr. Thomas B. Peacock, of Edinburgh, in an elaborate article upon the "Weight and Size of the Heart," in Reynolds's "System of Medicine," gives the mean weight of the healthy heart in males between forty and fifty years at nine ounces eleven drachms, or,

* Reynolds's System of Medicine, p. 368.

more generally speaking, estimates the *average weight* in males at *nine ounces eight drachms*. The same writer says, "In cases of obstructive aortic valvular disease I have weighed hearts ranging from fourteen ounces to twenty-one ounces in males. In cases of *aortic valvular incompetency* I have found the heart to weigh from fourteen to thirty-four ounces in males. . . . In the case of incompetency of the aortic valves it is often impossible to say how much of the great enlargement of the heart is due to the obstruction and how much to the incompetency of the valves, for the latter condition is generally only the final stage of the former." Farther on he says, "*In combined aortic and mitral valvular disease* the weight of the heart is intermediate between that which obtains in the two separate forms of disease, the organ being lighter than in aortic, heavier than in mitral, disease. In males, in cases of this kind, the heart was found to weigh fourteen ounces eight drachms to twenty-one ounces eight drachms."

DISCUSSION ON THE KERATOSCOPE.

Dr. Heyl thought that the instrument cannot be sufficiently accurate to be of practical value. It must give only a very gross idea of the corneal surface. It may, however, answer for class-demonstration. Helmholtz's ophthalmometer is applied with every precaution for exactness, the head is firmly fixed, and the apparatus arranged with the greatest precision. In the present instrument nothing of the kind is done, and incorrect observations would probably result. Practical experience with the instrument may show a more favorable opinion, however. As regards the determination of intraocular tension, we must confess that the subject is but little understood. It was a question with him whether abnormal tension over the sclera necessarily involved a corresponding change over the cornea. He referred in this connection to a case of glaucoma in which the tension over the cornea seemed to be diminished at the same time that the scleral tension was increased, and also to a case of conical cornea in which normal scleral tension was coincident with diminished corneal tension.

Dr. Landesberg said that the instrument is not offered in comparison with the ophthalmometer of Helmholtz, by which we can measure mathematically the degree of astigmatism, but which cannot be used in general practice, as it requires extended preparation and a great loss of time for observation in a single case. The keratoscope shown to-night can, however, be used very satisfactorily in general practice, and is sufficiently accurate for ordinary practical purposes. He recently had a case in which no disturbance of equilibrium of the ocular muscles could be detected by the usual method of examination, but there were double images, standing above each other, and the keratoscope showed that the left eye was deviated somewhat downwards. He does not use the instrument to measure astigmatism, but only to detect it by a preliminary examination. The instrument is easy to manage, and can be used to demonstrate as well as for actual observation. In some cases of regular astigmatism the rings appear as if they were drawn on a concave surface.

In determining intraocular tension the instrument is an aid, but is not offered as perfect test. The method of examining the tension of the eyeball by pressing the fingers directly on it is not a new method, but is the best, and was used long ago by Coccius. It is preferable to the method of making alternate pressure upon the eyeball through the closed lid, as its resistance may deceive the physician. This method should always be used. We have only to direct the patient to look upwards, and to warm our fingers before applying them on the globe.

A CONVERSATIONAL meeting of the Society was held at the hall of the Society on May 23, 1883. Dr. Charles W. Dulles read a paper entitled "Remarks on Hydrophobia" (see page 785).

DISCUSSION ON HYDROPHOBIA.

Dr. H. C. Wood, opening the discussion by request of the Chair, said: I consider the paper an extremely able production, but cannot agree with the conclusions at which the author arrives. Though much nonsense has been spoken and written about this subject, and almost every ridiculous theory has been advocated, yet we cannot reject the inference from certain facts which are well known. A disease undoubtedly exists among the *carnivora*, and especially among canines, which is propagated by biting, and attended with decided nervous manifestations and disturbance of the salivary apparatus. This disease has been passed from one animal to another hundreds of times by biting and by injection of saliva. Such results cannot be ascribed to the influence of fright, as they occur in lower animals which are incapable of continuous fright and of disease-simulation. In physiological laboratories animals are subjected to an endless variety of injuries, but no one has ever seen hydrophobia arise from these causes; but when we take a small amount of saliva from a rabid animal and inject it into a healthy one we have the specific disease developed in about sixty-six per cent. of the cases. When the virulent saliva is introduced into human beings by the bite of a rabid animal, we have a series of symptoms of analogous character. It must surely be regarded as very remarkable that out of say one thousand persons dying with hydrophobic symptoms a much larger proportion than nine-tenths are

found on investigation to have been previously bitten by dogs. Very few cases are known in which hydrophobic attacks have followed the bite of a dog not mad.

The pathology of the disease is involved in doubt. The lesions that have been detected in the brain are rather the effect than the cause of the phenomena. We would naturally expect that such violent action would give rise to the alterations which have been observed with the microscope.

The question as to the long incubation does not affect the main point as to the existence of the disease. Mistakes may have been made in some of the cases where very long incubation has been supposed and infection more recent may have been overlooked. We must remember that a lick or a very slight scratch may be the means of communicating the disease. In the case reported by me lately, no instance of a bite more recent than three and a half years could be found.

A striking case, almost positive proof of the correctness of the accepted views on hydrophobia, is detailed by Dr. Colin,—a case which was discussed before the French Academy and received the credence of Bouillaud. In 1865 a French soldier in Algiers was bitten by a rabid dog, and a comrade who rushed to his assistance was also bitten. Both wounds were cauterized by the hot iron. The first soldier died in a few days; the second experienced at the time no serious effects, but after an interval of five years died of acute hydrophobia. The case was officially reported on to the French government, because if it could be established that the death was due to the bite received in the attempt to rescue a comrade, the widow would be entitled to a pension. The most careful search failed to show any possibility of infection other than the one alluded to; and the case therefore remains as one of long incubation, though it is true that objection might be made that some unnoticed or forgotten introduction of virus had occurred more recently. It is not possible to explain why the virus remained so long inactive.

One of the great difficulties in the way of a thorough knowledge of rabies is the danger of investigating it. Even Dr. S. Weir Mitchell, although accustomed to play with rattlesnakes, had declined to undertake experiments on hydrophobia. Youatt is supposed to have committed suicide because he felt the beginning of the symptom of the disease after having repeatedly escaped from the effects of bites from rabid dogs.

Dr. Mills: I think Dr. Dulles has very well set forth the arguments in favor of the view that in man at least hydrophobia as the result of a specific infecting virus does not exist. One of the best demonstrations of this is the fact that a distinguished investigator was able in thirty years to obtain only three cases which could be considered hydrophobia. Of course the results of experiments on inoculation might be regarded as favorable to the theory of specificity; but it has been shown that hydrophobic symptoms may be produced in animals by the inoculation of the saliva of non-rabid animals. It is, however, the existence of hydrophobia in the human species that is most doubtful. In nineteen out of every twenty cases the disease is probably not hydrophobia at all, and in many instances, certainly in every Philadelphia case which I have seen or of which I have knowledge, the bite supposed to have caused the disease was by a dog not rabid. Every symptom in so-called hydrophobia is capable of being produced by other diseases,—*e.g.*, localized meningitis, tubercular tumors, tetanus, reflex epilepsy, acute mania. A case has very recently come under my notice in which from injury to the finger with a piece of brick tetanoid symptoms were developed, with well-marked sputtering of saliva, which is considered so typical a symptom of hydrophobia. In one case, which I saw in association with Dr. Burns and Dr. Wood, the autopsy revealed distinct pachymeningitis; and in another case chronic brain-lesions were found which were sufficient to account for the symptoms. The high percentage (eighty to ninety per cent.) of instances in which persons are bitten by rabid dogs and yet do not go mad is another point of importance. Even experimental inoculation succeeds in only about one-fourth the cases. As regards the influence of fear, I may mention the cases of two ladies, both patients of mine, who cannot endure the sight of a dog, and who are thrown into great excitement at the mere mention of the animal. In another case, a patient actually barked like a dog, spat, sputtered, and had clonic spasms, symptoms which wholly ceased on threatening to use the actual cautery.

That cauterization applied to recent bites has apparently been efficacious in preventing infection is no argument; for the same applications have been of great effect in epilepsy.

The long period of incubation is a serious difficulty. If we take vaccine virus as a typical virus, we can get an idea of how extraordinary the supposed long incubation of hydrophobia is. Vaccine loses its power within a few months at most. It seems very difficult to suppose that the virus of hydrophobia can remain for years locked up in the system and then suddenly break out with violence. The case of the soldier alluded to by Dr. Wood is not absolute proof of long incubation. Other infection is possible,—syphilis, for instance; and no proof is adduced that the dog which did the biting was mad.

Dr. Moon: The paper presented this evening is as gratifying as the remarks of Dr. Wood on "The Pathology of Hydrophobia" a few weeks ago were unsatisfactory,—this being a fair and creditable presentation of the subject, while Dr. Wood might have been describing a typical case of acute mania as

not infrequently seen. The pathological specimens exhibited could very readily be ascribed to a diseased condition of the brain which would induce mania.

There is already in the public mind too much misapprehension on this subject, which should not be encouraged by physicians.

Dr. Dercum: The virus-theory may be unsatisfactory, but it is the best. It is very difficult to see how mere reflex from afferent nerves could produce the symptoms observed in hydrophobia in dogs, especially the melancholia. Another fact in favor of the specificity of the poison is the frequent success in inoculation. Some dogs have been inoculated only after several trials; but these failures are dependent on differences in the susceptibility of the animals. The period of incubation is, it is true, very variable and often long; but then the periods of incubation of all diseases are subject to much variation.

Dr. William R. D. Blackwood: That there is such a disease in dogs I believe from experience, but as to its occurrence in the human species I do not know personally, although I do not believe that the thousands of physicians who have treated and described the disease before our time were all fools, or that the hundreds of people who have died from rabies were all victims of their imagination; neither do I believe that the most ardent admirer of the fright and imagination theory would deliberately allow a mad dog to bite him, and then depend upon his superior wisdom and lack of imagination to avert the possible and probable development of hydrophobia. In 1868 a rabid dog entered the camp where I was stationed, and, after snapping at various other dogs, he bit—*first*, a small lap-dog, *second*, a woman, and *third*, another small dog. The mad dog was then killed. The woman had two wounds, which completely penetrated the webbed portion between the thumb and index-finger of her left hand, and I at once cauterized the punctures by pouring nitric acid into them, *the acid passing freely through each* and dropping from the under side of them. The two bitten dogs were cooped up, and *both died* from hydrophobia; but the woman remained well when I last heard from her, ten years afterwards. The peripheral irritation from the severe bite or the severer nitric acid did not develop hydrophobia, yet there must have been virus present; for the animals bitten *before* and *after* her wounding died from rabies.

I also believe the disease to originate spontaneously in dogs. Some time after the occurrence mentioned above, a small cur-dog became offensive through biting strangers and visitors to the post, although he did not ordinarily bite those belonging to it. The dog was tied up securely for a time, and I believe he had abundance of water and proper food. He did not, however, have sexual intercourse, which may or may not have had anything to do with his case. This dog went mad; and I caused him to bite another animal for experiment. Both animals died. No rabid animals had visited camp since the time when the woman was bitten, and the dog now referred to was not then born: hence the inference plainly is that this case of rabies arose spontaneously.

I cannot see any difficulty about the virus lying long dormant in the system before breaking out. Syphilis, apparently eradicated, will show itself many years afterwards without a second inoculation. Diseases of all kinds require longer or shorter periods of development after exposure to them in different persons.

Dr. O'Hara: I do not think that syphilis is fairly comparable to hydrophobia, for the former is transmissible directly from one human being to another, but the latter is not. Possibly but little confidence can be placed in the decisions made from the superficial examinations of pension surgeons, and therefore the case of the French soldier which Dr. Wood had mentioned is not unobjectionable. Dr. Agnew and I saw a case which presented the typical symptoms of hydrophobia: Dr. Agnew considered it a brain-tumor, but the autopsy did not reveal any gross lesion. Many physicians would have put it down as hydrophobia, unacquainted with the previous history. The clinical symptoms of hydrophobia were very marked at one time, but the case closed with the symptoms of *acute paralytic dementia*. Hydrophobic symptoms are not very uncommon in many brain-diseases.

Dr. Bartholow: The disease hydrophobia admits of a large latitude of opinion, as has been well shown by Dr. Dulles's paper. Two groups of cases are presented. In one group imagination or morbid fear is the causative influence. All the objective symptoms of hydrophobia may be produced in this way. Persons who have been bitten by dogs brood and worry over the matter, closely study every symptom as seen in cases, until they develop a condition of the most serious character. The influence of the mind over the body under such circumstances is well illustrated by a case recorded in Tuke's work on the Influence of the Mind over the Body. Two brothers were bitten by the same dog, and shortly after this event they separated, one going to Belgium, the other remaining in France. The one in France was a few days after seized with hydrophobia, and died. The other, after a long absence, returned to France, learned for the first time of the manner of his brother's death, was shortly seized with hydrophobia, and died. Numerous cases have been recorded, in both sexes, having all the objective phenomena of hydrophobia, produced merely by mental causes, by fear, and by mimicry or morbid irritation. These constitute a distinct group. We have thus a neurosis having all the ordinary characteristics of hydro-

phobia. I am a firm believer, also, in the existence of a morbid entity, to which we apply the term hydrophobia, which is propagated by a distinct virus that is inoculable. We have knowledge of the fact that the saliva and other secretions may become toxic. Montgomery records a case in which an infant was poisoned by nursing at the breast of the mother while she was in a very high passion. Sternberg has shown that human saliva may become poisonous; and authentic instances are known of the communication of hydrophobia from man to man by bites given in states of tremendous passion. The recent discoveries in regard to the ptomaïnes, alkaloidal poisons produced during the decay of animal tissue, will throw some light on this subject. In the case referred to by Dr. Blackwood we have evidence of how close confinement and deprivation of customary indulgences can unfavorably affect an animal; and in the same manner a dog chased or annoyed in the street may be so affected that its saliva will become dangerous.

As regards the length of the period of incubation, we must remember that this period varies in all diseases. If the period of incubation is two weeks,—which is not unusual,—why not two months, or two years?

Dr. Hamilton: I recall the following case, which may be interesting in this connection. A man had fallen against a rail and hurt his shin severely, but continued at work for two or three days. The wound inflamed and suppurated, and then tetanoid symptoms set in. To these hydrophobic symptoms supervened, shuddering, and going into spasms whenever an effort was made to drink water. This condition continued about thirty hours, when death ensued. The patient had never been bitten by a dog.

Dr. Wittig: About forty years ago I saw a case in which a young man who had been sleeping with a dog found his mouth covered with foam, and foam on the mouth of the dog also. The young man was very much alarmed when I saw him, but he recovered under the use of calomel and diaphoretics associated with a remedy that is used in Russian practice,—the genista. I was told that vesicles had appeared under the tongue and had been cauterized. In another case, a young man was bitten in the finger, caustic was applied, but the patient got very much frightened and sent for me. I used remedies similar to those in the case just mentioned, and the patient got well. Dr. Heger, of Vienna, has said that in his opinion the saliva is not the cause of the disease, but it is a lesion of a nerve causing peripheral irritation. I incline, however, to the opinion that it is a virus, and that it may become latent and lie dormant for some time in the system.

Dr. W. S. Stewart: In Westmoreland County, in this State, lives a family of physicians who claim to have a remedy for hydrophobia, and people come to them from all points for treatment. In one case, a patient of my preceptor's, feeling symptoms of an alarming kind, went to one of these doctors, and received from him one pill, to be taken on arriving home. This pill on being opened was found to be made of bread-crumbs and to contain a piece of paper bearing the word *abracadabra*. This pill, taken with restriction as to stimulants and excitement of all kinds, constituted the treatment, whether the cases were real or imaginary.

Dr. Dulles, in closing the discussion, said: I will endeavor to reply to some of the points raised, in the order in which they came up; for time will not permit a systematic reply.

In the first place, I must call attention to the vagueness of some of the statements of Dr. Wood. His figures in regard to successful artificial inoculations are not to be accepted without question. If his statement that sixty-six per cent. of experimental inoculations of rabies are successful is true of the Laboratory of the University of Pennsylvania, then the success there is about four times as great as has been attained elsewhere. In the famous experiments of Hertwig, of Berlin, to which reference is always made in this connection, only about one-quarter were followed by what are called "positive results," and some of these were duplicated experiments on the same animal. Again, the statement that ninety-nine per cent. of cases of hydrophobia can be traced to the bite of a mad dog is also incorrect, even if one puts faith in testimony drawn out, as it often is, after suspicious symptoms arise. It is often the purest assumption to say that the dog that gave the bite was rabid. It is a necessary part of the inoculation theory, but it is not supported by good proof. A very common history of such cases is that a strange dog ran up, snapped at the person, and then ran away and was lost, nothing being known about it before or afterwards.

The story credited to M. Colin is very interesting, but it has been exploded. M. Colin himself told it, with reserve, to the French Academy of Medicine; and it fell to pieces under the criticisms of that distinguished body. It gives no support to the theory of long incubations, but demonstrates the credulity of those who accepted it.

In reference to Mr. Youatt's views, it must be remembered that although Youatt is worthy of much respect as a veterinarian, yet he is not much of an authority in regard to the diseases of men. In regard to hydrophobia he held some peculiar views. For example, he believed in the preservative influence of delayed cauterization, and claimed that he had saved the lives of four hundred persons bitten by rabid dogs by cauterizing their wounds with nitrate of silver. He was altogether too credulous. Indeed, too much reliance has been

placed upon the statements of the English veterinarians. Mr. Fleming, who is often quoted as an authority, and who wrote a book on this subject, recently acknowledged that he had seen in all his life only one case of hydrophobia, and that was not under his own care.

Dr. Blackwood, in his remarks, mentions an experience of his own in which a dog, being kept in restraint and being deprived of sexual indulgence, developed spontaneous rabies. Now, the testimony of the most reliable veterinarians is almost universally against the possibility of rabies arising in this way. Though I had not time to tell stories illustrative of the points dwelt upon in my paper, I will give one in this connection. M. Bouley, having long denied that there was any really trustworthy evidence of a spontaneous rabies, told the French Academy of Medicine, about twenty years ago, how he had recently thought he was mistaken. He had a bitch brought to him at Alfort by her owner, who said she was so valuable for breeding-purposes that she was never out of his sight. The last time she was in heat he refused her the male, and in consequence rabies developed. M. Bouley showed the animal to his students, and confessed that with such positive evidence this must be regarded as a case of spontaneous rabies. In three days the bitch died, and at the autopsy "what did we find," said M. Bouley, "in this beast whose heat had not been satisfied, according to the positive assertion of her master? Four young ones in her belly. Behold how they write the history —of bitches!"

The story of the peasant of Languedoc, related by Dr. Bartholow, is a very familiar and oft-repeated one in the literature of hydrophobia. It varies a little in different accounts. The place to which the second brother is said to have travelled is not always the same. But the most constant thing about it is that it is used in a way diametrically opposite to that in which Dr. Bartholow has used it this evening. It is generally cited as an illustration of the effect of the imagination, and not considered a very valuable argument in favor of long incubations. The statement that Dr. Sternberg has found human saliva to be poisonous "under *peculiar* circumstances" is not correct. The fact is that Dr. Sternberg has been but one of several experimenters who have demonstrated that inoculation of certain of the lower animals with human saliva proves poisonous under *almost* all circumstances. Again, Dr. Bartholow spoke of cases of communication of hydrophobia from man to man as "well authenticated." After investigating the subject for fifteen months, I must say that I have not found a single case of this kind which deserves, or gets, serious consideration. Nor is there any scientific proof that rabies can be excited by deprivation of sexual gratification. Some very cruel experiments have been performed in France with the object of ascertaining whether or not hardships, solitary confinement, and the deprivation of food, of water, and of sexual indulgence would cause the development of rabies, and the result has been always negative.

It has been said by Dr. Bartholow, in answer to objections raised against the credibility of very long periods of incubation, that these vary in different diseases, being longer in some and shorter in others; and that "if there may be an incubation of two weeks, why not of two months or two years?" This is a dangerous style of argument. One might as reasonably say, "Men vary in height, and if a man's height may be six feet, why not sixty feet?" The periods of incubation in diseases that are fairly understood are always reasonable. A period of incubation is not a period of total inactivity, but simply the time required for a chemical poison to make its way through the system, or for a vital poison to multiply until it is capable of producing certain gross manifestations. Consistently with this fact, the longest periods of incubation which are well established are found to be measured by days or weeks,—never by months or years. For incubations that exceed reasonable limits syphilis is sometimes cited. This has been done to-night. We are not here to discuss syphilis, or it might be interesting to consider the value of some of the assertions made in regard to it. But I call attention to the faulty method of arguing which this appeal to syphilis illustrates. One hypothesis is made to rest upon something else, which itself is nothing but an hypothesis. Unfortunately, many of the arguments in support of the commonly-accepted theory of hydrophobia are of this sort, and but little of the evidence upon which it rests can be regarded as having *an accuracy and a reliability commensurate with the delicateness and gravity of the questions at issue.*"

PATHOLOGICAL SOCIETY OF PHILADELPHIA.

THURSDAY EVENING, JUNE 28.

The President, Dr. TYSON, in the chair.

A specimen of renal calculus—Oxalate of lime.
By J. H. MUSSER, M.D.

A. R., æt. 32, a stone-cutter, residing one year in Philadelphia, applied to me for treatment on June 5, bringing with him a sample of his urine. It was bloody, and he had been passing it without pain during the twenty-four hours prior to this visit. Three months ago, without any cause, he was seized with pain in the left loin. The pain increased in severity, kept him from work, caused faintness, but no nausea or vomiting; it did not radiate in any direction, save transversely to a slight extent, lasted three days, and was not followed by hemorrhage. In the intervening

three months he was in good health. A jar of the body or any movement did not increase the pain. It may be noted, but is rather irrelevant, that for ten or more years he has always had a weak back, becoming painful when stooping.

Twelve hours after the consultation this same localized pain recurred, and continued for twenty-four hours. Suddenly it was relieved, and the subsequent urinary discharges were clear. In three days he had a return of the pain and hemorrhage simultaneously. The pain increased in severity, and at the height of the paroxysm it extended to the testis and to the head of the penis. The paroxysm was relieved by the passage of the calculus I show you, by the urethra. The passage of the stone through the ureter was characterized by the most agonizing pain and frequent attempts to urinate. First sample of microscopical examination of the urine showed the red color due to blood. A similar examination of his clear urine, as well as the bloody, proved the presence of urate of soda and crystals of the phosphates. The urine was acid. The diagnosis of renal calculus was not difficult, but the microscopical appearances of the urine led me to infer that the stone was composed of uric acid. With such idea the patient was put on alkaline treatment. The sequelæ proved that the saline effect of such treatment was of no avail, save from the diuretic action of the drugs. Morphia was given to relieve pain.

A portion only of the stone was received, an angular portion having been broken off by the patient. The portion weighs .06 grain. The exterior is tuberculated; the interior dark in color, bluish, the exterior lamina light-brown. It is exceedingly hard. Chemical examination was made by Dr. Leffmann, who reported it to be an oxalate of lime calculus.

Hæmatoma in the upper portion of the mediastinum producing death by suffocation. Presented by J. T. ESKRIDGE, M.D.

Dr. Eskridge said that he obtained the specimen from a colored man, porter, aged 32 years. He had led an irregular life, but it was not positive that he had ever contracted syphilis. During the past winter he suffered from a severe cold on the chest, and was told by his physician that his heart was inflamed. With the exception of slight cough, unattended by any difficulty in breathing, he had considered himself in excellent health on June 10, 1883, when, after carrying a heavy trunk on his shoulder from the first to the fourth floor of a hotel at Cape May, he was seized immediately with great difficulty in breathing, and was compelled to seek the open window to prevent suffocation. During the next two days, while still remaining at the sea-shore, he suffered from several attacks of shortness of breath, each lasting from several minutes to an hour or more.

He was admitted to the hospital of the Jefferson Medical College on June 12, about sixty hours after the occurrence of the accident. On admission his breathing was so labored that he was unable to speak. He was gasping for breath, and bathed in profuse cold perspiration. Respiration 36, with greatly prolonged expiration; pulse, 100; temperature, 99.7°. Inhalations of amyl nitrite and the hypodermic use of morphia and brandy seemed to give relief. The attack lasted about twenty-five minutes. He slept well during the night, and was tolerably comfortable until five P.M. the next day, when he had another paroxysm, which was promptly checked by amyl nitrite, morphia, and dry cups to the chest.

14th, A.M., Dr. Eskridge saw him for the first time. The patient was breathing quietly, and said that he felt comfortable. Pulse and temperature were nearly normal; urine contained neither albumen nor sugar. No cardiac murmur was detected. Lungs were hypo-resonant at their apices. Loud, moist, bronchial râles were present throughout both lungs. In view of the man's former freedom from attacks of dyspnœa, he thought that he detected nothing capable of giving rise to so great interference with respiration. The next two days no dyspnœic attacks occurred, and he seemed to be doing well.

17th. His breathing was distressing for several hours. Nothing seemed to afford much relief.

18th. Severe asthmatic breathing came on at six A.M., and lasted in its worst form about six hours. Chloral hydrate gave some relief. After that paroxysm his breathing did not again become quiet. He died exhausted at seven P.M. the next day. With the exception of occasional headache, he did not complain of pain at at any time.

Sectio cadaveris made by Dr. Parrott, the medical resident.

Thorax.—Pericardial sac was completely obliterated by old firm adhesions between the pericardium and heart. The heart was rather soft, and both ventricles were relaxed and contained considerable dark fluid blood. Valves nearly normal. Both pleuræ were adherent to the upper portion of the pericardium, and the left pleura was everywhere adherent to the lung. Both lungs were emphysematous at their apices, and considerable lobular emphysema existed. Bronchial tubes were congested, and contained considerable mucus.

A semi-solid, or rather soft, oblong body, about two inches long by one and a half wide, was found lying upon the lower anterior surface of the trachea, embedded in connective tissue and firmly held by old adhesions. It occupied a position just above and behind the transverse portion of the arch of the thoracic aorta. Three of the rings of the trachea in the position where greatest pressure was

exerted by the semi-solid mass presented a dark color, and one of the spaces between the discolored tracheal rings was nearly ulcerated through from the effects of pressure. On cutting into the tumor it was found to consist of blood more or less clotted. The calibre of the trachea was greatly lessened at the seat of hemorrhage. No ruptured blood-vessels were found. No disease was observed in any of the arteries. The abdominal organs were nearly healthy in appearance.

Commenting on the case, Dr. Eskridge said that numerous cases of rupture of the aorta, or of smaller blood-vessels, into the trachea, bronchi, œsophagus, or mediastinum were on record, but in all of them, so far as his knowledge went, death resulted directly from loss of blood. The peculiarity of the case of which he had given a description was the formation of a hæmatoma in the mediastinum. In hemorrhages into that space the blood usually gravitated to the lower portion of the chest, and the patient soon died from loss of blood. In the case he presented, however, on account of extensive, old, and firm adhesions of pleuræ, pericardium, connective tissue, and everything else in the upper portion of the mediastinum, a hemorrhage in that situation must necessarily have been circumscribed, and could have taken place only gradually by dissecting up the adhesions. He thought that the condition of the parts that prevented an extensive hemorrhage predisposed the smaller blood-vessels, especially the veins, of that locality to rupture. In view of the extensive alterations by means of general adhesions that had taken place at the seat of hemorrhage, it was not surprising that rupture of a blood-vessel should have occurred when the parts in the anterior region of the neck and upper portion of the chest were suddenly put upon the stretch, as occurred in the act of raising a heavy trunk from the floor and placing it upon the shoulder. From the specimen, as he obtained it, he was unable to say whether the hemorrhage had occurred from a rupture of the aorta or one of the smaller vessels, or from the bursting of a very small vessel that had become aneurismal. The tearing across of small veins would have been sufficient to give rise to the extravasated blood.

Fatty congenital tumor beneath occipito-frontal muscle. Presented by Dr. NANCREDE.

The tumor was simply presented on account of the rarity of such growths of *fœtal origin*. The patient was a child aged 16 months. The growth was noticed at three weeks, and in consequence must have been of fœtal origin, as it then was of the size of a bean. It presented none of the symptoms of a fatty tumor, except a faint lobulation, the skin moving freely over it, and presenting none of the dimpling so common in lipomata. Its site was peculiar for a dermoid cyst,—viz., over the right occipito-parietal region,—yet its resemblance was so close that by exclusion it was considered to be a congenital cyst. Upon removal it was seen to be beneath the aponeurosis of the occipito-frontal muscle. Microscopic sections, kindly made with the freezing microtome by Dr. W. G. MacConnell, showed that the growth was a pure lipoma.

Adenomatous growth apparently recurrent, in reality an outlying portion of the mamma not removed at a previous operation. Presented by Dr. NANCREDE.

The above title really gives the essential points in the history of a patient, aged 23 years, upon whom two operations had been performed for a supposed fibroma (adenofibroma?) of the breast, which recurring after partial removal of the breast, the whole organ it was supposed had been then removed by another surgeon. Dr. Nancrede had opposed all operation at first, considering the breast was really not the seat of anything beyond a local induration after injury. He had removed the third growth, which from its history and microscopic appearances he was satisfied was the result of the irritating drag of the badly-placed cicatrix on a small portion of breast-tissue left at the second operation. The clinical lesson taught by this case was clear,—viz., the freest possible removal of mammary growths.

GLEANINGS FROM EXCHANGES.

NON-TRAUMATIC PERIPHERAL NEURITIS.—During recent times perhaps the most promising and interesting paths of neural pathology have been those relating to the influence of the nervous system in the production of nutritive lesions of almost every tissue of the organism. This is brought out not only in the large number of recorded instances of such affections, but also in the remarkable speculations which have been adduced to explain their occurrence. In choosing a site wherein to locate the essential or proximate cause of such lesions, none seem so inviting on physiological, almost instinctive, grounds as the central ganglia of nerve-cells. Charcot selected the large cells of the anterior cornua as the seat of the lesion which presided over the affection of bones and joints associated with the phenomena of locomotor ataxy. Buzzard speculated on the existence of a "centre" in the floor of the fourth ventricle which governed the tissue-changes of joints, and pointed to the frequent association of gastric crises with arthropathies as supporting such an hypothesis. Bärensprung, Charcot, and others have regarded the ganglia on the posterior root as operative in the causation of herpes zoster or shingles, and certainly with greater probability of truth

as compared with the previously-mentioned conjectures. It will be remembered that Jonathan Hutchinson has suggested that some cases of trophic lesion may be due to a morbid state of the peripheral vaso-motor nerves. In a series of articles now appearing in the *Archives de Neurologie*, MM. Pitres and Vaillard are contributing papers with the object of establishing the importance of peripheral nerves in the origination of the various nutritive lesions. The present number of the *Archives* contains an account of two cases which are given in support of their views. The first instance happened in a patient the subject of leucocythæmia. Towards the close of life an area of anæsthesia, which was nevertheless the seat of much pain, appeared on the chin, lower lip, gums, and a part of the buccal mucous membrane, followed by ulceration of the lower lip. At the autopsy an examination, directed only to those parts of the inferior dental nerves after their emergence from the dental foramina, showed that there were no healthy fibres; the myelin sheath was segmented into droplets and granules, the nerve-tubes were partially atrophied and deformed with varicosities, some fibres were so much changed that their interior contained nothing more than yellowish granulations strewn with nuclei. We do not think it justifiable to draw any inference from this case; the insufficient examination of the nervous tissues apart from those mentioned, and the possibility that the alterations of the nerves discovered were secondary to changes, not necessarily ulcerative, of the tissues involved, are, we think, considerations of much weight. The second observation was made in a case of progressive locomotor ataxy, with chronic œdema of the left lower limb and arthropathy of the left knee-joint; the post-mortem examination revealed typical sclerosis of the whole of the posterior columns of the spinal cord, with changes in the peripheral branches of the left sciatic nerve. It may not be unimportant to mention that the right sciatic nerve, although apparently examined by the same mode of preparation, revealed no morbid signs. Many fibres of the left sciatic nerve exhibited degenerative changes like those which have been named Wallerian, after the great experimenter on nerve-degenerations; the posterior articular nerve to the left knee-joint, as well as the muscular nerves, presented similar lesions. The coexistence of the arthropathy with the degeneration of the nerve is certainly important; but, as with other examples of even constant association, the interpretation is by no means easy. M. Déjerine has recently described degenerative lesions of the sensory nerves of the skin supplying a patch of anæsthesia in an ataxic patient. Atrophy of the optic nerve is familiar in tabes dorsalis; although associated, it is probably not continuous in structure with the sclerosis of the spinal cord. In connection with these considerations the frequent occurrence of transient nervous symptoms, usually ascribed to a flying neuritis, in the earlier periods of tabes dorsalis is noteworthy. —*Lancet.*

THE INFLUENCE OF CALOMEL ON DIGESTION.—Dr. Vassilieff (*Zeitschrift für Physiolog. Chemie*) has found, from experiment, that the presence of calomel, at least up to the amount of five grammes, in the alimentary canal, does not interfere with the gastric juice, nor affect the triple influence of the pancreatic fluid on albumen, fat, and starch; on mixing the latter fluid with fibrin and calomel, the formation of certain products, indol, etc., always appearing as a result of prolonged digestion under normal circumstances, is prevented. The gases generated in the process of pancreatic digestion contain none of the usual products of fermentation and decomposition when calomel is present; sulphuretted hydrogen and pure hydrogen are absent; carbonic acid is diminished to from two to ten per cent., whilst under natural circumstances from fourteen to fifty-four per cent. is found in the gases evolved by the action of the pancreatic fluid. In fact, calomel prevents all other changes in nutritious substances save those produced entirely by the digestive secretions, decomposition and retrogressive processes in albumens being entirely checked. Calomel also prevents butyric acid fermentation, as Vassilieff found by experiments on cheese. The action of calomel readily explains the cause of the green color of fæces passed by patients to whom that drug has been administered. Hoppe-Seyler rightly attributed this coloration to the presence of unaltered bile. Now, under normal conditions, bilirubin and biliverdin are changed, by a process of decomposition, into hydrobilirubin, and thus become no longer recognizable in the excretion; but this process is arrested by calomel, and the coloring agents, unaltered, give the fæces their peculiar bright-green hue.—*British Medical Journal.*

STRANGULATED OBTURATOR HERNIA RELIEVED BY OPERATION.—Dr. Hasselwander, of Hausheim, Bavaria, records the following case in the *Aerztliches Intelligenzblatt* of successful operation for strangulated obturator hernia. A countrywoman, 65 years of age, complained of colicky pain, constipation, and flatulence, and on two occasions vomited. She also had pain in the left foot, and the thigh was swollen, and the entire extremity was numb. On close examination a fulness was detected in Scarpa's triangle, and pressure in this place elicited pain. On deep palpation, an indistinctly-circumscribed, hard, smooth swelling was found on the inner side of the femoral vessels, over the adductor longus. On vaginal examination, fulness

could be detected on the left side of the pelvis. Taxis having failed, an incision was made from below the pubes three inches along the border of the adductor longus. This muscle was then cleared by dissection and drawn inwards, partly exposing the pectineus. The swelling, about the size of a pigeon's egg, was now exposed. The sac did not contain fluid; the intestine lay immediately against its inner wall, which consisted of thick œdematous tissue, easily lacerated; its outer layer was aponeurotic. The intestine was congested and very tense. The neck of the sac was found with the finger, and the constriction divided with a straight probe-pointed bistoury, and the hernia was reduced. There was considerable venous hemorrhage from the wound. Notwithstanding recovery was delayed by suppuration of the wound, the patient, in six weeks, was entirely restored to health.—*British Medical Journal.*

THE ACTION OF SODIUM SALICYLATE ON THE HEART.—Prof. Maragliano, of Genoa, has investigated this subject afresh, the results of previous observers being in many points contradictory. Some make out that the heart is weakened, others that it is strengthened, by the salicylate. Liebermeister, for instance, in Ziemssen's Cyclopædia (vol. i.), says he never gives the salt to a patient whose heart is weak, and he advises that it should never be prescribed in such cases. Maragliano made a triple series of observations to determine the question. In the first, patients who were taking the salicylate regularly were examined sphygmographically before, during, and after the exhibition of the dose, morning and evening. In the second, the pulse-curve of other persons was taken before and after the exhibition of a single five-gramme dose. In the third, the arterial pressure was measured by Basch's method before and after the exhibition of a single five-gramme dose. The results showed that—(1) As the dose was gradually increased in patients of the first class, the pulse became progressively stronger, and the systolic line of the sphygmographic trace became higher. (2) After the single dose the pulse was stronger and the systolic line higher; the increase appeared an hour after the dose, reached its maximum in two to three hours, and disappeared after three to five hours; the normal dicrotism was accentuated, and often passed into tricrotism. (3) The arterial pressure rose about an hour after the exhibition of a single dose, and returned to the normal about three hours afterwards. The rise in pressure varied between ten and twenty millimetres of mercury. These results indicate unmistakably that no depressing influence is exerted on the heart by the salicylate.—*Centralbl. f. Med. Wiss.; Practitioner.*

THREE SUCCESSFUL CASES OF NEPHRECTOMY are reported by J. Knowsly Thornton (in the *Lancet*, May 26),—in a child of 7 years, a married woman of 26, and a widow of 58 years, respectively. The first was one of hydronephrosis, the second a scrofulous pyonephrosis, and the third a nephritic abscess due to pyelitis following impaction of a calculus in the ureter. The second case had probably a similar origin. In the operation Listerian precautions were carefully observed, and the operator especially lays stress upon the advantages of the lateral incision over the ordinary one in the lumbar region. He also especially recommends securing the stump of the ureter in the abdominal wound, so that it may not drain into the peritoneal cavity. The operation of Langenbück has so many advantages that he would not return to the loin-operation with its difficulties and uncertainties. He is inclined to recommend an exploratory incision by the lateral abdominal section, with careful Listerian precautions, in any case in which it is of importance to examine thoroughly the kidney and ureter. In one of the cases symptoms of carbolic intoxication appeared, which compelled a substitution of a borated dressing. All the patients made good recoveries.

PARALDEHYDE: A NEW HYPNOTIC AGENT.—Mr. John Brown writes to the *British Medical Journal* that he has used with satisfaction this new drug on several occasions. From thirty to fifty minims constitute the dose for its hypnotic effect. He says that it produced sleep, in most cases, in a few minutes after administration, lasting from three to seven hours. In one case a slightly-depressant effect upon the heart was observed. It produces a peculiar burning sensation in the mouth after taking it, otherwise no disagreeable effects were noticed. Its action is very much like chloral, but at present it is about sixteen times as expensive, otherwise it might prove a useful substitute.

MISCELLANY.

THE LANCET ON THE NEW YORK CODE.—In a recent editorial the *Lancet* notices Prof. Flint's recent brochure on Medical Ethics and Etiquette, and warmly supports its teachings. Speaking of the New York Code, it very justly condemns it in the following language:

"It is well known that a somewhat sharp difference of opinion has arisen among our transatlantic brethren lately on the question of professional intercourse with homœopaths. Though the principles of medical ethics are good for all time, new questions of application arise. Homœopathy is an instance. It is a thing of yesterday. It is not so much as mentioned by Percival, and the profession has had to deal with it as a novelty. Hitherto the profession has been practically unanimous

in refusing consultation with homœopaths, either because their practice was based on an exclusive dogma which is contrary to professional ethics, or on the ground, as Dr. Flint puts it, 'of assuming a distinctive appellation,' implying an essentially distinct system of practice and an attitude of antagonism to the regular professor. Dr. Flint does not so much object to a man holding any dogma, even an exclusive one, even one so absurd as homœopathy, as to his assuming 'a distinctive appellation.' This is very much the same ground as that taken by our own College of Physicians, which called upon its members to uphold the dignity and freedom of the profession by discountenancing those who trade upon designations implying special modes of treatment. Our own view is that, of two men practising homœopathy, the one calling himself a homœopath and the other not, the former is the honester man of the two. But it is not 'for the good of the patient,' which is the great end of medical ethics, that we should meet either of them. Such a consultation is misleading to the patient, and likely to be compromising to both parties.

"The New York Medical Society has altered, as is well known, the National Code of Medical Ethics, which declares that 'no one can be considered a regular practitioner or a fit associate in consultation whose practice is based on an exclusive dogma, to the rejection of the accumulated experience of the profession,' etc. It proposes to authorize consultation with any registered or qualified practitioner, whatever the absurdity or exclusiveness of his dogma, and whatever may be the trade label that he adopts. It is beyond the power of the New York Medical Society to impose such consultations on men who respect themselves or their patients, or the accumulated experience of the profession; and we shall be much mistaken if, under the presidency of Dr. Flint, the American Medical Association does not uphold its own moderate and dignified definition of a regular practitioner, as quoted above. Free institutions are admirable, but they must include freedom for those who decline to be warped by a dogma, or compromised by one who believes, however honestly, in an absurdity."

PRACTICAL ANTI-VIVISECTION PROTEST.—The Paris correspondent of the London *Lancet* gives the following interesting incident:

"It will be recollected that some three or four months ago Prof. Brown-Séquard submitted a paper to the Academy of Sciences giving the results of some experiments he had performed on some animals with carbonic acid gas, which he applied to the larynx, and thus produced local anæsthesia, with the view of facilitating certain operations on that organ, and without causing any pain to the subject. On Tuesday last, while the professor was giving practical demonstrations of these experiments on a monkey, in the presence of a rather large mixed audience, at the College of France, a rather sensitive lady, moved with indignation, left her seat, went straight to the Professor, and struck him a blow with her umbrella, which, of course, rather astonished him, but, without losing his equanimity, he had the lady removed from the amphitheatre and given over to the police. Here she endeavored to establish that the law for the protection of animals was on her side, which the Commissary of Police admitted for the time, but he did not see that this justified her conduct, and that, at any rate, she had no right to take the law into her own hands. She is therefore to be tried for the assault, and it is anticipated that this will give rise to new legislative measures for regulating the practice of vivisection in this country. It is to be remarked that the medical journals are rather silent on the incident, but the local newspapers have taken it up, some being for and others against vivisection; but all are unanimous in asserting that if it is necessary for the furtherance of science it need not be made a spectacle of, and should be confined to members of the profession. In France, however, this would be somewhat difficult to manage, as all lectures in the Government institutions are open to the public; but what may be done is that in similar circumstances as the present the lecturer may request oversensitive persons to leave the amphitheatre. Professor Brown-Séquard deserves great credit for his perseverance, and, nothing daunted, he is determined to continue his interesting experiments. He has this time selected monkeys for his subjects as being the nearest approach to man in their conformation, and thus hopes that in sacrificing a few more monkeys he will render service to humanity."

FISKE FUND.—The Trustees of the Fiske Fund, at the annual meeting of the Rhode Island Medical Society, held at Providence, July 21, 1883, announced that they had made no award on the subjects proposed for the present year.

They propose the following subjects for the year 1884:

1. The origin and progress of the malarial fever now prevalent in New England.
2. Original investigations in household hygiene.

For the best essay on either of these subjects worthy of a premium they offer the sum of three hundred dollars.

Every competitor for a premium is expected to conform to the following regulations,—viz.:

To forward to the Secretary of the Trustees, on or before the first day of May, 1884, free of all expense, a copy of his dissertation, with a motto written thereon, and also accompanying it a sealed packet, having the same motto

inscribed upon the outside, and his name and place of residence within.

Previously to receiving the premium awarded, the author of the successful dissertation must transfer to the Trustees all his right, title, and interest in and to the same, for the use, benefit, and behoof of the Fiske Fund.

Letters accompanying the unsuccessful dissertations will be destroyed by the Trustees, unopened, and the dissertations may be procured by their respective authors, if applications be made therefor within three months.

JOB KENYON, M.D.,
OLIVER C. WIGGIN, M.D.,
HORACE G. MILLER, M.D.,
Trustees.

CHARLES W. PARSONS, M.D., Providence,
Secretary of the Trustees.

DEODORIZERS AND DISINFECTANTS.—Dr. W. E. Buck, having used the whole round of disinfectants (carbolic acid, sanitas, terebene, resorcin, creasote, boroglyceride, chloride of zinc, charcoal, etc.) for cancerous ulcers without allaying the fetor or keeping the ulcer clean, tried a saturated solution of hyposulphite of soda added to an equal quantity of water, and found it exceedingly efficacious. The ulcerating surface was well syringed and washed with the solution, and was then covered with rags steeped in the solution. The granulations were kept clean, and the fetor was well kept under. Most disinfectants seem to lose their virtue after a few days' application, but he had used this one for months in the same patient with continuous good effects. It is cleanly, has no smell, does not stain, and is very cheap.—*British Medical Journal.*

THE ST. LOUIS MEDICAL SOCIETY, fearing that a preamble and resolution offered at the recent meeting of the American Medical Association by Dr. Atwood might cause misunderstanding as to its position with relation to the Code, passed the following resolution at its meeting in June last:

"*Resolved*, That the St. Louis Medical Society distinctly repudiates the statements contained in said preamble, and again expresses its fealty to the existing Code of Ethics as a time-honored and most suitable fundamental law of the profession, and specially deprecates any action calculated to reflect upon its loyalty to those principles which have heretofore secured immunity from the machinations of schismatics within or enemies without."

ORIGIN OF BOTHRIOCEPHALUS LATUS.—Braun, of St. Petersburg, claims that the pike is the source of the broad tape-worm, just as the pig is the source of tænia solium. Medium-sized pikes may contain as many as forty or fifty worms. Out of sixty pikes examined for them, there was only one in which traces of the bothriocephalus were not discovered in the muscle.—*St. Petersburg Medicinische Wochenschrift.*

SORE NIPPLES.—Dr. Favre (*St. Petersburg Medicinische Wochenschrift*) is of opinion that there are two varieties of these, fissures and erosions, and believes that the latter are to a large extent due to tight-fitting dresses and pressure by corsets. He advises that the nipples be sprinkled with bismuth, dry, or that this be made into an ointment in the proportion of one of bismuth to two of vaseline. This procedure has often resulted in a cure within twenty-four hours.—*Gaillard's Medical Journal.*

MR. W. F. PHILLIPS advocates, in a recent communication to the *British Medical Journal*, the use of belladonna in hay-fever. He reports a case in which one minim every hour of the succus belladonnæ yielded entire relief to the symptoms.

THE *Louisville Medical News* has made a change in its editorial staff. Dr. L. S. McMurtry has retired, and is succeeded by Dr. H. A. Cottell, who formerly was an editor of that journal.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY FROM JULY 28 TO AUGUST 4, 1883.

SUTHERLAND, CHARLES, COLONEL AND SURGEON, MEDICAL DIRECTOR MILITARY DIVISION OF THE PACIFIC AND DEPARTMENT OF CALIFORNIA.—The leave of absence granted by S. O. 64, Head-quarters Military Division of the Pacific, June 30, 1883, is extended two months. S. O. 168, A. G. O., July 23, 1883.

BAILY, JOSEPH C., MAJOR AND SURGEON.—Assigned to duty as post-surgeon at Fort Concho, Texas. S. O. 87, Head-quarters Department of Texas, July 19, 1883.

TURRILL, H. S, MAJOR AND ASSISTANT-SURGEON.—Granted leave of absence for one month, to commence September 1, 1883. S. O. 77, Department of the Platte, July 26, 1883.

APPEL, A. H., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Granted leave of absence for two months, with permission to apply for an extension of one month. S. O. 30, Head-quarters Military Division of the Atlantic, July 20, 1883.

LIST OF CHANGES IN THE MEDICAL CORPS OF THE NAVY FROM JULY 21 TO AUGUST 4, 1883.

P. A. Surgeon J. D. GATEWOOD detached from the "New Hampshire," and ordered to hold himself in readiness for sea-service.

Assistant-Surgeon PHILIP LEUCH detached from the Receiving-Ship "Franklin," and ordered to the "New Hampshire."

Assistant-Surgeon HORACE B. SCOTT ordered to the Receiving-Ship "Franklin," Norfolk, Virginia.

Surgeon D. MCMURTRIE detached from the Receiving-Ship "Franklin," and granted sick-leave.

P. A. Surgeon ROBERT WHITING granted one month's leave.

Surgeon J. W. COLES ordered to the Naval Hospital, Philadelphia, Pennsylvania.

P. A Surgeon E. H. MARSTELLER detached from the United States Steam-ship "Hartford," and granted sick-leave.

P. A. Surgeon J. F. BRANSFORD detached from the Naval Academy, and ordered to the "Hartford."

Medical-Inspector E. S. BOGERT detached from the Navy-Yard, Norfolk, on August 30, and ordered to the United States Steam-ship "Trenton," September 1.

P. A. Surgeon N. MCP. TIREBER detached from the Navy-Yard, Norfolk, on August 30, and ordered to the United States Steam-ship "Trenton," September 1.

P. A. Surgeon J. D. GATEWOOD ordered to the "Trenton."

Surgeon B. H. KIDDER detached from the Naval Station, Port Royal, and ordered to the Navy-Yard, Norfolk, August 30.